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XUM

THE

JOURNAL OF SCIENCE.

AUGUST, 1881.

I. THE CENTRE OF GRAVITY OF THE EARTH, AND ITS EFFECT ON ASTRONOMICAL OBSERVATIONS.

By Col. A. W. Drayson, R.A., F.R.A.S.

ANY novelty brought forward in the present day ought deservedly to be submitted to every form of fair criticism, in order that its truth or falsity may be tested. To assume that we have arrived at the end of discovery is, it will be generally admitted by reasonable men, a mistake; whilst to refuse to investigate, or even examine, any original problem, is a proceeding similar to that which retarded the advancement of Science in the Dark Ages.

During some considerable time I have endeavoured to make known through the usual channels the facts which I herein bring to notice, but from circumstances over which I have no control my endeavour has been thwarted. As, however, the facts remain, I believe it possible that many enquirers may be found who will be interested in, and may follow out, the proofs which I here submit to them.

At the present time it is assumed that the Earth is an oblate spheroid; that the Equator divides the Earth into hemispheres; that the two Poles of the Earth are equidistant from all parts of the Equator; that the axis of the Earth passes through the centre of the circle termed the Equator; that the zenith of all localities on the Earth is that point found by drawing an imaginary line from the centre of the Earth through the locality, and producing this line to the sphere of the Heavens; and that the various

zeniths correspond to the radii of a circle drawn from the centre of this circle or sphere.

On the accuracy, or otherwise, of these assumptions depend the correctness, or otherwise, of some important details in practical Astronomy.

An investigation of any terrestrial globe reveals the fact that, in the Northern Hemisphere of the Earth, the mass of land above the sea exceeds considerably the mass of land in the Southern Hemisphere that is above the sea. In the Northern Hemisphere, above the sea is the whole of Asia, Europe, and North America, two-thirds of Africa, and a small portion of South America. To balance, as we may term it, this amount of land in the Northern Hemisphere, there is in the Southern Hemisphere one-third of Africa, the island of Australia, and the greater portion of South America. The fact exists, therefore, that there is in the Northern Hemisphere at least twice the quantity of land above the ocean that there is in the Southern Hemisphere.

Now it does not alter the conditions or laws which will be referred to, even though the Earth be a perfect sphere instead of a spheroid. But it must follow that the surface of the ocean in corresponding latitudes, north and south, must be at the same distance from the Earth's centre; thus the surface of the ocean in, say, 10° S. latitude is at the same distance from the Earth's centre as the surface of the ocean in 10° N. latitude, and so on for each corresponding degree of latitude north and south of the Equator. It follows, therefore, that if the Equator be equidistant at all parts from the Poles of the Earth, the preponderance of land in the Northern Hemisphere must cause *the centre of gravity of the Earth to be located north of this Equator.*

Again, the distribution of land above the sea in the Northern Hemisphere is so arranged that the preponderance is located between about 15° W. longitude from Greenwich and 120° E. longitude, and a meridian of about 15° E. longitude passes over the greatest amount of land. It would follow, therefore, that if the Poles of the Earth be equidistant from all parts of the Equator, the axis of the Earth cannot pass through the centre of gravity of the Earth. If the axis of the Earth does pass through the centre of gravity of the Earth, then the poles of this axis cannot be equidistant from all parts of the Equator.

From a long series of investigations I have come to the conclusion that the centre of gravity of the Earth is located 1115 feet north of the Equator, and 2100 feet distant from

an axis, joining the two poles. A meridian of 15° E. longitude passes through this centre of gravity.

Before entering into a description of the results which follow, it must be pointed out that if the North and South Poles of the Earth be equidistant from all parts of the Equator, it must be a most marvellous coincidence if the axis of the Earth passes through the centre of gravity of the Earth, because the land and water on the Earth's surface are most unequally distributed. If the axis of the Earth does pass through the centre of gravity of the Earth, then the Poles cannot be equidistant from all parts of the Equator.

It may appear, at first sight, to those persons unacquainted with practical astronomy and geometry, that so small a divergence as 1115 feet for the position of the centre of gravity of the Earth from the Earth's centre would not affect, in a perceptible manner, any astronomical observations. The effects, however, would be most singular and most marked.

The zenith of any locality on Earth (from which zenith measurements are made to determine the zenith distance of stars, and hence their declinations) is obtained by means of a trough of mercury, or a plumb-line; whichever method was adopted the results would be the same, and for simplicity of explanation I will select the plumb-line.

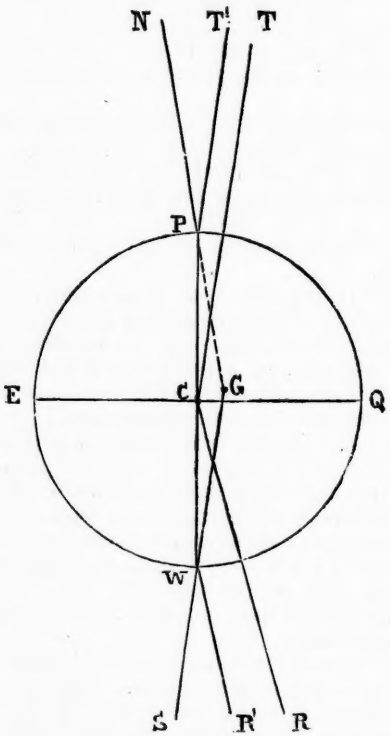
The plumb-line at any locality would be directed *to the centre of gravity of the Earth, and not to the true centre of the sphere*, and this law would hold good for every locality on Earth. The zenith of each locality, therefore, would be assigned a position in the Heavens found by drawing a line from the centre of gravity of the Earth through each locality, and producing this to the sphere of the Heavens. The results of this fact are most singular, and a few of these will now be demonstrated.

I will first suppose two observers to be located one at each Pole, and using a transit instrument. The Poles being supposed equidistant from all parts of the Equator, each observer ascertains the position of his zenith by means of a plumb-line or trough of mercury, and then determines the zenith distance of certain stars. Each observer being at a Pole of the Earth, zenith distances and polar distances would be supposed identical, and the effects on the supposed polar distances of stars can now be demonstrated.

E P Q W represents a section of the Earth through the Poles C W; C the centre of the Earth; G the centre of gravity of the Earth.

C T and C R the direction of two stars, the sum of whose declinations equal the angle T C R. The declination of the star T would be obtained by subtracting its polar distance from 90°, and the declination of the star R obtained in the same manner.

DIAGRAM I.



The observer at the North Pole, P, would find his zenith in the direction of G P produced, G being the centre of gravity of the Earth, whereas the true zenith of P would be in the direction of C P produced. From P draw P T′ parallel to C T, and the zenith distance of the star T would, by observation at the pole P, be N P T′. Consequently this zenith or polar distance would by observation be too great by the amount of

the angle C P G, which angle is the divergence produced on the plumb-line at the pole P, in consequence of the centre of gravity of the Earth not coinciding with the Earth's centre. In like manner, at the opposite pole W, a star R would be assigned a polar and zenith distance, S W R′, which polar distance would be too great by the amount of the angle C W G, which angle is the amount of divergence of the plumb-line, at the pole W, due to the centre of gravity of the Earth not being coincident with the Earth's centre. The declination, consequently, of the two stars T and R, derived from these observed zenith distances, would be too small by the amount of the angles C P G, C W G.

I will now suppose that an interval of six months elapsed, and the two stars, T and R, were again observed when the Earth had moved round 180° of its orbit; half a rotation of the earth having occurred in addition to the rotations due to six months.

The centre of gravity (G) of the Earth would, in consequence of half a rotation of the Earth, be located on the opposite side of the axis to that on which it was located six months previously; consequently the zenith at the Poles would be directed towards a point in the Heavens different from that point towards which it was directed six months previously.

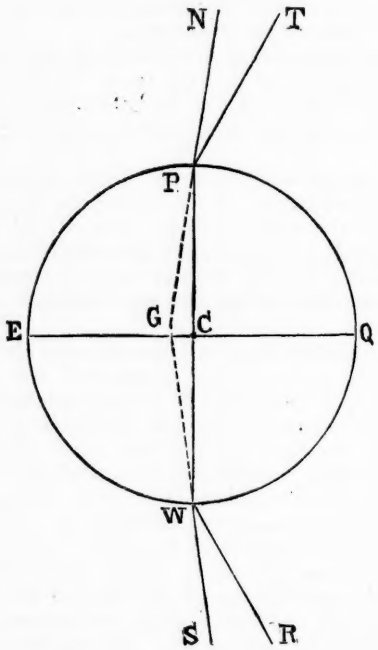
The following diagram (2) will demonstrate this fact.

As before, the points P and W represent the North and South Poles; E C Q the Equator; C the centre of the Earth; G the centre of gravity; P T the direction of the North Star; W R the direction of the South Star. A line from G through P will give, if produced, the direction P N for the zenith of P; and a line from G through W will give, if produced, the direction W S for the zenith of W. Consequently the observed zenith distance of the star T will be less from this observation than it was six months previously by twice the angle G P C; and the zenith distance of the star R, observed from W, will be also less than it was on the previous occasion by twice the angle G W C. But zenith distances at the pole are also polar distances, and consequently the assigned polar distances of the stars T and R would be less by twice the angle G P C when observed at the two intervals. If these two stars, having say eighteen hours' right ascension, the one a North Polar star, the other a South Polar star,—were observed at 6 a.m. in March, and again at 6 p.m. in September, the assigned polar distances of these stars would be found to differ considerably at the two dates, and consequently the sum of their declinations would be found

to differ considerably at intervals of six months, and from no other cause than that the centre of gravity of the Earth does dot coincide with the true centre of the Earth. The amount in minutes and seconds of arc due to this position of the centre of gravity of the Earth can now be demonstrated.

From an investigation of the relative arrangement of land and water on the surface of the Earth, I have concluded

DIAGRAM 2.



that the centre of gravity of the Earth is 2100 feet from the axis joining the two poles, which two poles are equidistant from all parts of the Equator. A second of latitude may be taken as equal to 101 feet on the Earth's surface; consequently 2100 feet may be taken as about 20″·7. It follows, therefore, that a plumb-line at the Poles directed towards the centre of gravity of the Earth would form an angle of 20″·7 with an axis joining the two poles. Consequently the

angle G P C in Diagram 2 would be 20″·7, and this 20″·7 is the amount that the polar distance of a star would be in error if seen from the Pole; and twice 20″·7, that is 41″·4, would be the difference in polar distance of a star found by two observations at intervals of six months. As this error would be nearly the same at both poles, it would be found that two stars near each pole would, when the sum of their declinations was taken at intervals of six months, differ to the amount of about 82″.

The question now arises whether any such differences have been found between any two stars when the sum of the declinations is taken; and observations prove that this fact exists, and has long been known, though a very different theory has been invented to account for the facts; it having been assumed, on no evidence whatever, and in spite of the facts shown by the unequal distribution of land and water on the Earth's surface, that the axis of the Earth passes exactly through the centre of gravity of the Earth, and that the poles of this axis are equidistant from all parts of the Equator.

I will take, for example, the two stars 4 Draconis and β Chamæleontis, whose declinations are given in the "Nautical Almanac of England and America." These two stars are nearly on the same meridian of right ascension, and their declinations have been found as follows:—

4 Draconis	78° 16′ 30″·4 N.,	Jan. 1, 1880.
β Chamæleontis ...	78° 38′ 33″·7 S.,	Jan. 18.
Sum	156° 55′ 4″·1	

In June these declinations are as follows:—

4 Draconis	78° 17′ 1″·2 N.
β Chamæleontis	78° 39′ 22″·5 S.
Sum	156° 56′ 23″·7
	156° 55′ 4″·1
Difference	1′ 19″·6 = 79″·6.

Or exactly the result which would occur in consequence of the centre of gravity of the Earth not coinciding with the Earth's centre.

Again, take the two stars Polaris and α Eridani, and, adopting the same principle, a difference of 75″ is found in

the sum of the declinations of these two stars between January and June of the same year.

Again, take the two stars σ Octantis and δ Ursæ Majoris, and compare the sum of the declinations of these two stars on March 30th and September 30th, 1880, and these will be found as follows :—

σ Octantis	89° 16′ 12″·7 S.,	March 30.
δ Ursæ Majoris ...	86° 36′ 11″·7 N.,	„
Sum	175° 52′ 24″·4	
σ Octantis	89° 16′ 47″·1	Sept. 30.
δ Ursæ Majoris ...	86° 36′ 51″·7	„
Sum	175° 53′ 38″·8	
	52′ 24″·4	
Difference	1′ 14″·4 = 74″·4.	

It thus appears that those differences actually occur in observations which must occur in consequence of the centre of gravity of the Earth not coinciding with the Earth's centre, causing a divergence in the zenith of certain localities, in consequence of the plumb-line being directed to the centre of gravity of the sphere, and not the true centre.

An examination of the Diagrams 1 and 2 will show that the great difference between the observed zenith distances of stars will occur at or near the pole, and when stars can be seen both above and below the pole. If a star can be seen only above the pole, the changes in zenith distance or declination, due to the position of the centre of gravity of the Earth, will be but slight. For example, supposing an observer at Q, Diagram 1, the zenith of this observer would be found by the production of the line joining G and Q, but a star near the zenith of Q could only be seen when the centre of gravity (G) occupied the same, or nearly the same, position that is shown in Diagram 1. Consequently the observed change in zenith distance of a star near the Equator, due to the position of the centre of gravity of the Earth, will be very slight, whereas the change in zenith distance of a star near the poles will be very great, as shown by Diagrams 1 and 2. Hence the greatest changes found annually in the declination of stars will be in circumpolar stars.

If the axis of diurnal rotation of the Earth were always parallel to itself, the zenith of an observer at the poles would

describe a circle during one rotation. The zenith of other localities would describe ellipses, which ellipses would show a greater difference between the major and minor axis the nearer they approached the Equator, until at or near the Equator the zenith would trace a straight line, equal in length to the major axis of the ellipse.

As, however, the axis of the earth changes its direction annually, to the amount of about 20″·25, towards or nearly towards the first point of Aries, the circle becomes a curve, which I have named the Nautilus curve. If it be a fact that the position of the centre of gravity of the Earth produces the apparent changes in the zenith distance of the stars, it will be possible, by aid of this Nautilus curve, to measure the amount of change in declination of any star near the poles, and also to show where the right ascension of *any* star will change, and how this change will vary during each month of the year. Such a demonstration must, I think, carry with it a proof of its accuracy.

(To be continued.)

II. GEOLOGY AS A SCIENCE AND AN ART.*

By C. Lloyd Morgan.

OF the several questions which my subject involves I give precedence to the very general one, "What is Science?" Now-a-days we hear not a little of science, of scientific culture, and of scientific discoveries. But I have no doubt that, if we were to ask a number of people of average education to set down a definition of science, many of them would be puzzled what point or points to pick out, by which to distinguish scientific knowledge from other kinds of knowledge. And I think that not a few of them might be driven to writing down a list of the several branches of science, and stating that science has to do with chemistry, geology, natural history, metallurgy, &c., &c. That is to say, they would define science by its subject matter. But we hear now-a-days of the science of history, the science of language, even the science of religion, while the science of sociology claims as its domain all the

* Read before the South African Philosophical Society in Cape Town.

facts which are connected with the social relations of human beings. I do not think, then, we can thus define science by a reference to its subject matter, since science now claims as its subject matter the whole realm of fact.

Etymologically science is knowledge; but we can hardly say that, as we now use the words, all knowledge is science. Let us suppose that an English schoolboy spends most of his holidays at the Zoological Gardens. At the end of four or five weeks he may know the names—perhaps even the Latin names—of a great number of the animals, and may have learnt also something of their habits. Now such knowledge, *as long as it consists merely of isolated facts*, is not science. But if he be an intelligent boy he *will* learn some science in his four or five weeks at the Zoological Gardens, for he will perceive that the animals are not placed in different cages *hap-hazard*. He will notice that some are grouped together, as the flesh-eaters or Carnivora; others, as the Rodents or gnawers; others, as the Ungulates or hoofed animals. If once the boy grasps this, his knowledge is no longer one of isolated facts, but a knowledge of grouped facts. This grouping of the facts is an essential element in Science. And much as a zoologist's knowledge differs from our supposed schoolboy's in extent, or number of facts known, it differs far more in the orderly grouping of the facts, in the perception of connection among the facts, and in the power of seeing their relations. In a word Science is organised knowledge, while mere general information is unorganised. This, then, is one of the important elements in Science. A second and equally important element is its exactness. Ordinary knowledge is indefinite and vague; Science is, or should be, definite and precise. For ordinary knowledge it is sufficient to say that all heavy bodies fall to the earth, while light bodies rise; but for Science this will not do, and since the time of Newton we must state the matter precisely, taking into consideration the mass of the attracting and attracted bodies and their distance asunder. For ordinary knowledge it is sufficient to divide plants into herbs, shrubs, and trees; but for Science the classification of the vegetable kingdom must be based on more exact principles. For ordinary knowledge, once more, it is sufficient to divide the higher animals into birds, beasts, and fishes; Science, however, makes use of more exact means of distinction than is afforded by the element in which animals live. And in some respects Science even reverses our ordinary rough-and-ready classification. It tells us, for example, that a whale, though it live in the sea, is not a

fish, but a beast; and that a bat, though it fly in the air, is a beast, and not a bird. For the whale and the bat agree with the higher beasts, or Mammalia, in the important characteristic of suckling their young. If, therefore, I were asked to pick out the two most distinctive features of Science, I should take *organisation and exactness*.

In further illustration of this, compare our ordinary knowledge of the heavenly bodies with that formulated by Astronomy. To the ordinary observer the sun shines by day, and by night the moon rises and sets, has her periods and her phases; Venus or Jupiter is now the first among the stars to appear in the evening, and now the last to be lost in the more powerful light of day; Saturn and Mars may be seen from time to time; Orion's belt, Cassiopeia, the Great Bear, and a few other constellations, are well-known groups; for the rest the heavens are more or less a maze. To the astronomer, on the other hand, every planet is but a unit in a *definite system*; of that unit the periods of revolution round the sun, of rotation on its axis, the figure, mass, and path, the motions of its attendant satellites, perhaps, and a dozen other facts are *accurately* known. The sun is not only our source of light, but the ruler of a system and the originator of nearly all available forms of energy on the earth's surface: he is not fixed in space, but drifting in a known direction and with a calculated velocity: he is not merely a ball of fire, but a body of wonderfully complex physical constitution and of ascertainable chemical composition. Nothing in fact can better illustrate the gradual advance in the organisation and exactness of scientific knowledge than the history of astronomy in general, and of the solar system in especial.

In organisation and exactness, then, I repeat, lies the essence of Science; or, to use another metaphor, scientific knowledge is *crystallised knowledge*, as opposed to the comparatively formless or amorphous mass which constitutes general information. What, then, is the aim of Science? Clearly to crystallise our knowledge; clearly to give ordinary knowledge that organisation and exactness which it lacks. And the aim of the several divisions of science, or sciences as we call them, is to develop a consistent body of crystallised knowledge, which we may call doctrine, concerning the particular groups of natural facts with which they severally deal.

And now let us see whether the various branches of science are all of one kind; for if they be not, then we shall have to note presently to which kind Geology belongs. If we

take a steam-engine, and try and find out the laws of its working, we shall, I think, find that there are three kinds of science involved. In the first place we may study its mode of working as a whole; that is to say, we may regard the steam-engine as a complicated mass of mutually dependent parts, and find out all we can about the working of these several parts under ordinary conditions, and as tending to the working of the engine as a whole. That is one way of studying it. Then, secondly, we may pay special attention to certain definite parts of the engine, and ascertain how they work, not under ordinary conditions, but under simpler conditions when distracting influences have been removed or allowed for. For example, we know the pressure of the steam and all the other conditions of its action on the piston, and we find out how rapidly this would propel the engine *if there were no such thing as friction.* This kind of science, it will be seen, differs from the first; its generalisations deal, if I may so say, not with things as they *are*, but with things as they *might be*. But for all that it aids us immensely in the study of things as they are. Thirdly, we may express numerically and calculate mathematically the rates of working of various parts of the engine, or the relative number of revolutions which the larger and smaller wheels make in the same time; that is to say, we may apply certain mathematical laws of relation to certain facts connected with our engine. But these laws of relation, which are worked out by the third kind of science, are altogether abstract, and have no necessary connection with the wheels or their revolutions, any more than with reapers and fields, or steamers and sailing ships, with the vast distances of stars or the minute masses of molecules, or any other problems to which they may be applied. The third kind of science is of no practical use by itself, but of enormous practical use in its application.

Of this third kind of science, to which Mathematics and Logic belong, I need say nothing here. Of the second class Physics and Chemistry are examples. Take as an instance of a scientific law of this class the well-known first law of motion,—that a moving body if left to itself would continue its motion for an indefinite time in the same direction and with the same velocity. But no real body that we know anything about *is* "left to itself." So that this is, if I may again be allowed the expression, a law of things as they might be, not of things as they are. Take, again, the law of pressures,—that the volume of a gas varies inversely as the pressure put upon it. Not only does that law require

careful allowance to be made for any variations in temperature, but, so far as I know, it does not strictly hold good in the case of any single known gas. At great pressures all gases show deviations from this law; but this is only because they are not under all conditions *perfect gases*. For a perfect gas, could such be found, the law would hold good for all pressures. Coming now to the first class of science (in the order in which I have enumerated them), we may take Zoology, or the natural history of animals, as an example. Naturalists study the animal world as they find it; and Mr. Darwin's law of Natural Selection has for its object to formulate how varieties and species *are* being formed under the conditions of our Earth, not how they would be evolved if those conditions were different. Botany, Sociology, Astronomy (at least under one of its aspects), and our special subject Geology, also come under this class of science.

Now all these three kinds of science are mutually helpful to each other; and now-a-days sciences of the first class, such as Geology, cannot get on at all without the aid of such sciences as Physics and Chemistry, which belong to the second class. Nor can the sciences of the second class get on at all without the aid of the sciences of the third class, Mathematics and Logic.

The next question is, Are these three kinds of science equally exact? To this I answer, No, they are not. The third, with Mathematics for its type, is by far the most exact. Pure mathematics is, indeed, the type of exactness and definiteness. Then comes Physics, the most exact science of the second class. Physics we may say is quite exact, wherever we may be sure of isolating its problems and getting rid of, or allowing for, all disturbing elements. Then it can apply mathematical processes with perfect confidence. But when we come to problems belonging to the first class of science, to the study of phenomena *as wholes*, then we cannot and must not expect the same exactness. This is not because the phenomena are not subject to law, for all phenomena whatsoever are subject to law, but because the conditions are so complicated, and there are such a number of influences at work, that the human mind is inadequate completely to unravel them. Even in Astronomy, where the conditions are by far more easily unravelled than in any other concrete science, the theory of the moon is still an unsolved problem.

I dwell on this distinction between these three kinds of science, partly because I believe the distinction to be in itself

very important, and partly because I want to make it clear that we cannot expect mathematical exactness in such a science as Geology, which it must be remembered belongs to our first class. In Geology, in fact, and in the allied sciences, the advance of our knowledge has, perhaps, been greater in the direction of organisation than in that of exactness. And with respect to this increased organisation it may be noticed that not only have geologists been able to class their stratified deposits, according to their mode of origin, into rocks formed by (1) mechanical agency, (2) by chemical agency, and (3) by vital agency; not only have they ascertained the order of the sequence of the strata; not only have they begun to grasp the definite succession of life-forms; but they have traced the shading of the stratified deposits into metamorphic rocks, these again into plutonic rocks of the granitic type, and these once more into rocks of volcanic origin.

But though geological knowledge is, compared to Mathematics, inexact, it does nevertheless become more exact, as any one may see who will take the trouble to compare our present knowledge with that of half a century ago; but we must not expect mathematical exactness. As we shall see directly, Geology is an historical science; it deals with the past. And if we cannot apply mathematical processes with success to the more complicated of the phenomena of to-day, the conditions of which are actually present, still less can we apply them to the past, when the conditions have to be inferred.

There is a well-known controversy between geologists and physicists, in which the distinction between the two kinds of science should be steadily borne in mind. The discussion is on the question of geological time. The physicist says to the geologist "The earth's crust solidified and cooled so many years ago," and brings forward in proof certain definite figures obtained by mathematical processes. "Geologists," he says, "must confine their speculations to this period." The geologist answers, "I can't bring forward figures, but that period is not nearly long enough." There the matter rests to-day, and, doctors differing, the general public does not know what to think. But I fancy that the potency of figures inclines them to the view of the physicist. Now the point to which I wish to draw attention is this—that it is the business of the student of physics, dealing as he does with a science of our second class, to isolate certain facts by excluding or allowing for all interfering causes. He considers each of the factors of phenomena separately; he

is accustomed, too, to be able to determine the data for the application of mathematical processes to his problems with great accuracy; so that in his own department his conclusions have a value that none can dispute. But suppose that he takes up such a problem as geological time, the difficulties of disentangling the factors of the phenomena and the difficulties of determining accurate data are so great that his conclusions cannot be said to possess anything like the same value. Let me, however, quote the words of Dr. Whewell, who, as both a mathematician and an historian of science, was peculiarly fitted to offer an opinion:—"A mature consideration of the subject," he says, "will make us hesitate to ascribe much value to the labours of those writers who have applied mathematical reasoning to geological questions. Such reasoning, when it is carried to the extent which requires symbolical processes, has always been, I conceive, a source not of knowledge, but of error and confusion; for in such applications the real questions are slurred over in the hypothetical assumptions of the mathematician, while the calculation misleads its followers by a false aspect of demonstration."

Now the geology of to-day cannot, as I have said, get on at all without the aid of chemistry and physics. For all aid which she receives from the cultivators of these sciences she is heartily thankful. But she does not like to have figures, evolved perhaps on insufficient data, crammed down her throat, nor does she like the tone of the remark "Geologists must confine their speculations to this period."

Leaving this point, let me now proceed to ask another question—"Is there a clear distinction between a Science and an Art; and, if so, of what nature is that distinction? I believe that there is a definite distinction. The object and aim of Science is, as I believe, to establish a body of organised, and as far as possible exact, knowledge: the object and aim of Art is to *apply* the knowledge thus gained to *practical ends*. I think this is a distinction which aids us much in getting clear ideas, and I shall therefore illustrate it by an example. It is the object of the science of Chemistry to ascertain the laws which regulate the combinations and recombinations of the several elements of which all the various substances with which we come in daily contact, of which everything, in fact, that is found in the crust of the earth, is composed. In the course of his work the chemist has to group these various elements, and to separate them one from the other. This he does as a scientific man, in order to gain knowledge. But he may not improbably come to the

conclusion that pure science does not pay; that is not at all an unlikely conclusion for him to come to, and he may therefore make it his profession to apply his knowledge to practical ends. He may become an *assayer*, and connecting himself, say, with the Cape Copper Company, may determine for them the amount of copper and other materials in their ores. Now I call assaying an Art, not a Science, and I think the assayer should be considered a professional man, not a man of science; for the man of science applies his knowledge to the widening the boundaries of our knowledge, or making our existing knowledge more completely organised or more rigidly exact, not immediately to the practical ends of every-day life. In the same way the navigator, the land-surveyor, the engineer, the medical practitioner are men who have to learn some of the facts and principles established by Science, for the sake of their professional work; but unless they are extending the boundaries of our knowledge, or contributing to the organisation of that knowledge, they are not men of science, in what I believe to be the true sense of the term. Nevertheless they will be successful in so far as they employ the method of science: that method is *to proceed by observation and experiment, by guarded hypothesis and careful verification from the known to the unknown, on the well-founded assumption of the uniformity of Nature.* Let me give you an example of the application of this method. The lead obtained from certain English and other ores contains a varying quantity of silver, often several ounces to the ton. Up to the year 1833, however, no method was known by which lead, containing less than 8 ounces of silver to the ton, could be desilverised. Thus not only was a large quantity of valuable metal lost, but the lead itself was rendered by the silver harder, and therefore for certain purposes less valuable. In 1833, Mr. H. L. Pattinson, among others, was endeavouring to solve this problem of the separation of these two metals, and it chanced that he dropped a crucible containing molten lead rich in silver. Such an accident might happen a hundred times and nothing come of it. But Mr. Pattinson's keen eye detected crystalline grains in the spilt metal, and he carefully picked some of them out for special examination. I do not know what thoughts were in his mind when he did so, but he may not improbably have said to himself something of this sort:—"When sea-water freezes the ice formed is comparatively free from salt, while the water remaining is comparatively rich in salt; it may be that these solid grains are comparatively free from silver, while the lead remaining is comparatively rich in silver." If he argued thus

he formed for himself an hypothesis. At all events he set to work and carefully analysed the crystals, and found as an actual fact that they *were* nearly pure lead. Taking for granted, then, the uniformity of Nature, Pattinson saw that what had taken place by an accident in his laboratory could be reproduced of set purpose on a large scale in a metallurgical process; and by the process he thus devised, which is called after him Pattinson's process, thousands of tons of lead are now being treated every year. This discovery of Pattinson's affords us, as it seems to me, a good example of the employment of the scientific method.

And, now, after so long an introduction, an introduction, however, not unnecessary, I come to my more immediate subject—Geology as a Science and an Art. First, what is its aim as a science? Secondly, what arts depend upon the body of doctrine which it establishes? As a science geology occupies rather a peculiar position. If we separate it from that which is now called physiography, but which is also called the science of physical geography, it becomes a purely historical science. Hence it has been said that geology is the physical geography of the past, and physical geography is the geology of the present. To understand what geology is, then, we must understand what physical geography is. Unfortunately physical geography, as set forth in many of the older text-books not yet out of use, is a queer jumble of fragmentary information. We all know the celebrated definition of a man of culture as one who knows something about everything and everything about something. I think the books on physical geography, of which I speak, may be defined as containing a little about everything and everything about nothing. But if we take physical geography in the relation to geology above indicated, we must define it as the science which treats of those causes now in operation, which modify the crust of the earth. Then geology becomes the application of the knowledge thus gained to the past history of the earth under the altered conditions indicated by a study of such portions of the earth's crust as come within our reach. For such study mineralogy and petrology afford some of the requisite data.

There is another branch of science which, in one of its aspects, is an integral part of geology, and, in another of its aspects, is a portion of the science of biology. I refer to Palæontology. Palæontology is the study of ancient life forms. It gives facts concerning the creatures which once lived on the earth's surface, which facts may be interpreted

on the principles of biology. Just as mineralogy and petrology give facts concerning the minerals and rocks which are buried in the earth's crust, which facts may be interpreted on the principles of physical geography.

Now, if we turn to Sir Charles Lyell's master work on geology, first published more than fifty years ago, we shall find that its title runs as follows: "Principles of Geology; or, the Modern Changes of the Earth and its Inhabitants Considered as Illustrative of Geology" (11th edition). In that book, therefore, and it is still and will long continue a standard work, we may say that the Science of Geology is, by implication, defined as the ancient physical and life history of the earth. The physical history is based on the application of our knowledge of those causes now in operation, which modify the crust of the earth elaborated by the science of physical geography. The life history is based on the application of our knowledge of the facts of animal and vegetable life elaborated by biology. This is the widest definition of geology. But from what I have said it will, I think, be seen that it is quite impossible to circumscribe a definite area of knowledge, and say, this is geology and nothing but geology. I have, therefore, prepared a table showing the manner in which geology is related to her sister sciences.

MATHEMATICS AND LOGIC.

CHEMISTRY AND PHYSICS.

Mineralogy and Petrology. (for facts.)	Physical Geography. (for principles).	Biology. (for principles.)	Palæontology. (for facts.)
	PHYSICAL. GEOLOGY.		PALÆONTOLOGICAL.

In that table are shown the two sub-divisions of geology, the one dependent on mineralogy and petrology for facts and physical geography for principles, the other dependent on palæontology for facts and biology for principles. Chemistry and physics, which are placed above, deal with the isolated factors, of which the problems of the sciences below are the complex products; while mathematics and logic, which are placed at the top, deal with the relations which may be established between the facts or phenomena supplied by the underlying sciences.

With the principles of all the branches of science, which are included in that table, the geologist should be acquainted. Of course I do not mean to say that he should attempt to keep abreast of all these branches of science. On the con-

trary, if he is to advance our knowledge of the subject he must pick out a small plot of the wide field of geological science, and rather spend his time in cultivating that thoroughly, than in wandering up and down that and adjoining fields, and doing a bit here and a bit there. If he be a man of genius he may in time be promoted to be an overseer. But it requires very exceptional powers to be a geological philosopher like Sir Charles Lyell, not only surveying but organising the whole field of geological science.

I may here, perhaps, be allowed to say a few words concerning the value of geology in education. In the University of the Cape of Good Hope, chemistry and geology are alternative subjects for matriculation. We may, I think, take it for granted that the vast majority of candidates are acquainted with no other branch of science, except of course the mathematics. Refer now to the table above. A text-book on geology deals more or less fully according to its scope, with all the branches of science set down on that table, except, of course, formal logic. A text-book on chemistry, on the other hand, deals with its own subject matter alone, *plus* some applied mathematics, and some physics, the first principles of which are generally so clearly explained that this is a clear gain. I leave you to judge which is the best for a candidate to take up. Wherever it is possible I should advise the student, who wants to gain some scientific culture, to study an example of each of the three kinds of science, say mathematics, physics or chemistry, and geology or natural history. From each a distinct kind of training is acquired; from the first, accuracy of thought and exactness; from the second, the application of exact methods of actual facts encountered experimentally, under conditions fairly within control; from the third, comprehensiveness, power of observation, and the invaluable habit of taking all the facts into consideration to the exclusion of none. If, however, he has only time to take up two branches, let them by all means be mathematics and physics or chemistry. With these as a foundation he will be able at any time to take up and make rapid progress in such a science as geology.

Leaving "Geology as a Science," let us now turn to "Geology as an art." [Here followed examples (1) of the loss of money which has so frequently been the result of sinking for coal in subcarboniferous rocks, in wilful ignorance of geological results, or even in spite of the warnings of geologists; and (2) of the practical good that has come of

the enlightened application of theoretical knowledge of geology, as in the case of the Artesian well at Grenelle, near Paris.]

As an art, then, geology has done great service to mankind, and in doing so she has profited as a science. Such a well as that of Grenelle, near Paris, would never have been sunk without the aid of the geologist. But in sinking such a well new facts are learnt and new inferences drawn, which may be built up into the fabric of geological doctrine. This is always the case when science and art work hand in hand. To take one other example among many, science gives to the world the art of telegraphy; and when telegraphy is established, science can fix with an accuracy otherwise impossible the longitude of such a place as the Observatory of the Cape of Good Hope, and thus add to the accuracy of her doctrine.

I now pass on to illustrate the methods and results of geological inquiry. I have said that geology is an historical science. The question therefore arises, How are we to solve such problems as the mode of origin of the rocks, seeing that they were formed in the remote past? There is an old method and a new, an unscientific method and a scientific. The old method was to sit in your study and *think the matter out*. Let me give you the results of one or two of these thinkings. One thinker—it is hardly necessary to mention the names of these excellent folk—one thinker imagined that the crust of the earth was fissured by the sun's rays, and thus the diluvial waters were let loose from a supposed central abyss, and from these waters the rocks—they are estimated to be about fourteen miles thick in England—were deposited in a few days. Another attributed everything to the tail of a comet, with which the earth was fortunate or unfortunate enough to come in contact. A third set down the formation of all the strata to the heaping up of materials thrown out by volcanoes. Then, again, with regard to the fossilised remains of organic beings. One said they were sports of nature, very like real shells indeed, but due to the influence of the stars or to fermentation. Another followed Theophrastes, the pupil of Aristotle, in saying that the fish had gone astray out of the rivers, and so—how much lies hid in that "and so"—and so got petrified. While a third maintained that the fossil shells on high mountains had fallen from the hats of holy pilgrims. We laugh at these idle speculations. But it is rather the futility of the method than its results that we should deride. I, for one, have no wish to disparage honest studious thought, but

I *do* wish to point out the necessity of balancing honest thought with a due weight of honest fact.

That, then, is the old method; what is the new? The answer to this question contains the golden rule of geology. *Read the riddle of the past by the light of the facts of to-day.* [The rest of the paper consisted of a description of ice-action in the past as illustrated by a careful study of the ice-action of to-day.]

III. THE SOURCE OF ELECTRIC ENERGY.

By CHARLES MORRIS.

(Continued from page 391.)

WHERE a charged conductor acts upon an uncharged body the same law is obeyed. If the conductor be positively charged it affects the molecules of the other body, the adjacent poles becoming negatively, the remote poles positively, charged. This primary action is followed by a secondary action of conduction and neutralisation between the poles of anterior molecules, and only the charge of the surface molecules persists, this being negative on the adjacent surface and positive on the remote surface. If, now, this remote surface be connected with the earth its charge disappears, the effect being simply an immense increase of surface. On this connection being again broken, the conductor remains in the apparently anomalous condition of possessing a negative charge on one surface and being destitute of charge elsewhere. It might be imagined that a new induction would take place through the neutral body, with a reproduction of its former state; but the fact is that, through the loss of the positive induced force, the negative induced force has become negative charge. The case has become changed from that of a positively charged body acting upon an uncharged, to that of a positive acting upon a negative charge. And this negative charge is just sufficient to balance the induction of the positive body. The inductive action continues, but the positive induced energy of the remote surface is exactly balanced by the negative

charge, so that this surface remains electrically neutral, and only the negative charge on the adjacent surface is apparent. On increasing the distance between the bodies the inductive effect diminishes, and negative electricity appears on the whole surface. On diminishing the distance the inductive effect increases, and positive electricity reappears on the remote surface.

This view of the case dispenses with the usual idea that positive repels positive, and attracts negative, and *vice versa*. Each conductor simply acts towards the other as it would upon an uncharged conductor. If the first be positive and the second negative, the first exerts an inductive influence upon the second, causing negative force to appear on the neighbouring, and positive on the remote, pole. This new force is added to or subtracted from the previously existing negative of the second, so that this energy is augmented on one surface and decreased on the other. The second body exerts a like influence, in the opposite direction, upon the first. In the case of both being positive, or both negative, the rule is the same. Each acts on the other as on an uncharged body. The charge on each has added to it an induced charge, which weakens it in one direction and strengthens it in the other. There is no real attraction of unlike and repulsion of like electricities, though the result gives that impression. In the interior of a charged conductor a similar rule holds good. If one surface be plane and the other concave internally, the lines of induction from the concave to the plane surface are divergent, those from the plane to the concave are convergent. Thus the inductive effect upon any portion of the concave surface exceeds that upon an equal area of the plane surface. The charge on any area of the concave surface is augmented, that on an equal area of the plane surface decreased, by this difference in inductive action. Thus the surface charge of a conductor varies in amount in strict conformity to the shape of the surface of the conductor.

The conclusions thus arrived at lead to another probable conclusion, at which we have already glanced. In a charged conductor the primary effect of neutralisation of the internal molecular charge would be to confine the effective electricity to the outwardly directed poles of the surface molecules. But the neutralisation of the opposite charge on the inwardly directed poles of these molecules must render this restriction impossible. The electric disturbance necessarily flows back and affects the molecule as a whole. Instead of its having a normal heat and two opposite abnormal atomic

vibrations, it has a normal heat and an abnormal electric vibration, whose pitch is either above or below that of the heat, according as the charge is positive or negative. Thus there is a surface layer of electrically charged molecules, acting inductively in both directions, inward and outward. Its outward inductions are effective; its inward are ineffective, except in the case of an irregularly shaped body, in which the inductive effect, through the body, on equal areas of the surface, differs in vigour.

The principle considered in the above argument is undoubtedly a universal principle of Nature, and one which arises from the necessary relations of the molecular condition of matter. Electric disturbance is not necessary to its action. Any discordance in the motive relations of molecules is sufficient to produce its effects. Every molecule disturbs the motive conditions of every other not actually homogeneous with it. There is a constant resistance to heterogeneity, and a constant effort to produce homogeneity, in the motive conditions of the molecules of all substances. But this effort is partly balanced by a counter effort in each substance to retain its normal molecular condition. Thus between the effort to produce conformity and the effort to resist change there is a constant struggle. The latter is usually the stronger when the bodies are separated; but when they are brought into contact the former may often prevail, even if there is no difference in their motive vigours, if we may judge by the apparent results of contact electricity. When there is a difference in vigour, whether this results from heat, from friction, or from chemical change, the effort to produce conformity usually, perhaps always, overcomes, with the production of the various electrical phenomena. Every molecule whose vibratory relations are disturbed causes a similar disturbance, gradually decreasing with distance, in the vibratory relations of surrounding molecules. Every molecule having a different vibratory period from that of any other seeks to produce conformity to its own condition in this other, and the disturbance may become measurable if they be in contact. Wherever the vibratory energy of the one exceeds that of the other, the disturbance is followed by a flow of energy, if resistance to this flow be not too declared. The principle is that every molecule strives to preserve its normal motive conditions, and if these be disturbed to restore them, either inwardly or by the aid of a like disturbance of the motive conditions of neighbouring molecules, and an outward neutralisation of the disturbed vibratory energy. These molecules, in their

turn, resist disturbance to their normal conditions, and there is thus a struggle between two opposed energies, of which the strongest must conquer. If restoration of normal conditions be produced inwardly, no result appears; if outwardly, a flow of electric force is the apparent result.

In the case of a conductor of galvanic electricity the conditions are really similar to those existing in static conductors. The flow of the current is here preceded by an inductive action in both directions, whose result is the appearance of free positive electricity on one pole and negative on the other. This inductive action rapidly increases as the energy of chemical action becomes more declared. Finally, the polarisation is so vigorous as to overcome the resistance of the dielectric medium, and the two electricities become neutralised in this medium, with the production of a concentrated local energy by their combination, vigorous enough to yield heat and light. Or if the resistance be too great for this, particles are torn bodily off from the poles, and conveyed across the intervening space, the energy which they carry becoming partly converted into heat in the passage.

The electric current here produced is not confined to solid conductors. It also passes through liquids,—either that concerned in the chemical action, or other liquids introduced at intervals in the current. And its passage through these liquids is accomplished in a mode which it is usual to consider as peculiar, and essentially different from its method of passage through solids. If, however, the hypothesis which I have here presented is a correct one, the method of electric conduction is the same in all cases, and there is nothing peculiar in electrolytic conduction. The electrolyte is made up of compound molecules. Conduction in it is undoubtedly preceded by induction, as elsewhere. The motive relations of its molecules are disturbed, one of the constituents of the compound becoming positive, the other negative. If, as in static electricity, the influencing cause be removed, these disturbed conditions re-combine in the molecule, its weakened chemical coherence is restored, and no change results. If, on the contrary, electric energy be produced, the disturbed conditions re-combine outwardly, the polar elements of each two adjacent molecules combining, with a neutralisation of their opposite electricities. In this case, then, the flow of the electric current is accompanied by a chemical disintegration and reintegration, which extends in lines from the point of chemical action to the conducting poles. No energy actually passes. The energy emitted by the chemical action is employed in neutralising the opposite electricity of the

adjacent molecule of the electrolyte. A like neutralisation proceeds throughout the electrolyte. The pole of the last molecule in contact with the solid conductor yields its free energy to this conductor, and that element of the molecule which constitutes this pole is set free. Thus at the poles of the electrolyte the chemical disintegration becomes effective. In this process the passage of energy is but from molecule to molecule. The energy emitted at the battery plate is only a counterpart of that yielded to the conducting wire. The electric influence has made its way across the electrolyte without any passage of a distinct volume of energy through the liquid.

In the passage of electric energy through a dielectric there is reason to conclude that a like decomposition occurs, though it is not succeeded by a recomposition in the above sense. For the appearance of ozone, when this dielectric is the atmosphere, indicates a separation of the oxygen molecule into its constituent atoms, and a subsequent combination of these atoms with other oxygen molecules. Perhaps the resistance to chemical disintegration and direct reintegration may be an essential cause of the resistance to electric conduction. A similar action is indicated by the supposed production of nitric acid in the air through the agency of the lightning-flash.

If, then, in all cases in which we have an opportunity to trace the mode of electric conduction, it presents this character, we certainly have great reason to conclude that this is its essential mode, and that the passage of electricity through a solid conductor is performed in the same manner. It is certainly highly improbable that electricity has two distinct and widely different modes of movement, and far more probable that it is conducted in the same manner where we cannot, as where we can, trace its movement.

It may be, then, that in all cases the electric conduction of energy proceeds as follows :—An inductive disturbance of molecular motion precedes the current. The molecules become positively electrified at one pole, and negatively at the other. The influence which produces this polarisation acts to prevent a re-combination of the molecular energies inwardly, and tends to produce such a re-combination outwardly. The adjacent poles of every two contiguous molecules being respectively positive and negative, the constituents of the molecules separate, and the opposite poles of each two adjacent molecules combine, their diverse motions also combining into normal heat-vibration. In non-conductors this disintegration is vigorously resisted, and

there may be no immediate integration. In imperfect conductors, as in the electrolytic liquids, it is less vigorously resisted, and immediate production of new molecules occurs. In good conductors, as in the metals, resistance is greatly reduced, and a chemical change, like that in electrolytes, may take place with great readiness. And it is not improbable that the matter which passes from pole to pole may be set free as a final resultant of this process of chemical change. It is, however, not impossible that in these good conductors the neutralisation of the disturbed force may take place by a vibratory swing of molecule to molecule, each yielding energy to neutralise the disturbed condition of the next, without any necessary disintegration.

The essential point, however, is that there is no actual passage of energy throughout the circuit at the speed of the electric current. The real transfer of energy is comparatively slow, if it exists at all, and the energy which flows into one end of the circuit is simply balanced by an equal volume of energy which is set free by the molecules at the other end of the circuit, or at intermediate points in the circuit where the local transfer is resisted. There is, in this method of transfer of energy, a close analogy to the action of a row of elastic balls, of which, when the first is struck, the last starts into motion without any visible disturbance of the intermediate ones. The motion here is transferred from ball to ball by a series of elastic repulsions, and the motion displayed by the last comes from that immediately adjoining it, and in no exact sense from the first.

The above hypothesis indicates a close relation between chemical change and electric action. Chemical change takes place between heterogeneous molecules, which, at every approach, produce this inductive disturbance in each other. Such a disturbance weakens the cohesion of the constituents of the molecules. As the molecules approach each other the electric polarisation increases, and cohesion decreases. When very close the cohesion of the elements of the molecules may give way, and new molecules form in consequence, with the production of a new chemical compound. But in every such case there must be an electric current, through the best available conductor, this being ordinarily the material immediately surrounding the combining elements; and there must be a production of heat at some point in or throughout this circuit, in exact accordance with the energy set free in the chemical action.

The process of electric conduction here indicated finds significant analogies in certain phenomena of the current.

In a galvanic battery, for instance, the current produced is only that of the terminal cells of the battery. The internal cells add nothing to the volume of the current. Each of these, in fact, sends positive energies in one direction to meet the negative sent in the opposite direction by the next cell. Neutralisation, therefore, takes place between every two cells, and only the energy of the terminal cells flows over the wire. Thus each two cells act in the same manner as each two molecules in the above hypothesis, and the energy produced by the chemical action in each two becomes heat in the intermediate conductor. The only advantage of a many-celled battery is that each assists in the polarising action, so that the polarisation of the intermedium becomes great, and resistance to electric transfer correspondingly decreased. A like condition exists in the thermo-electric circuit. The current is only that of the terminal pairs, each intermediate pair acting the part of two intermediate molecules in a circuit. But increase in the number of pairs adds to the polarisation of the molecules of the circuit, and therefore to the electromotive force.

We have, thus, the following significant indications of the real process of electric conduction. In the case of static electric charge there appears to be molecular neutralisation of the electric disturbance internally, so that only the surface charge persists. In the galvanic current that portion which passes through compound liquids does so without any definite transfer of energy, but by a series of decompositions and recompositions, there being thus an internal neutralisation of the electric disturbance, the motive energy which appears at one end of the circuit not being that which entered at the other end. In the passage of the current through the atmosphere we have significant indications that the process is the same as the above, in the production of ozone as a resultant of chemical change. Other indications which point to the same conclusion we have just seen to exist in the action of the galvanic battery and of the thermo-circuit. There is no complete electric circuit through the battery. Neutralisation of the electric disturbance takes place between each pair of cells, while final neutralisation of the electric disturbance caused by the terminal cells takes place through the conducting wires. The same rule holds in the thermo-battery. Thus, instead of there being any complete electric circuit, there is a series of successive neutralisations of the opposite electric energies between every pair of active nodes in the circuit. In the case of the atmospheric and the electrolytic

portions of the circuit we perceive that this process of neutralisation takes place between every two adjacent molecules, the positive and negative polar energies of each being neutralised by a process of reintegration, and only the energy of the terminal poles of the molecular circuit being yielded to the solid conductor. Are we not, then, justified in concluding that the current passes through the solid conductor in the same method that successive molecular neutralisation takes place with possible reintegration, and that the energy which appears is the energy set free by the poles of the terminal molecules? This is certainly far more probable than that there are two distinct and unrelated methods of electric conduction.

The next point of importance in this connection is the production of heat in the circuit. This heat is precisely equal to that which would have been set free at the point of chemical action had there been no electric circuit. The energy yielded by the chemical action is simply transferred by the electric movement, to appear as heat throughout the circuit. And the quantity of heat which appears at every point in the circuit is in exact accordance with the resistance to polarisation at that point. If the total resistance of the circuit exceeds the total polarising energy no current can pass; but the instant the polarisation exceeds the resistance the current passes, and the heat produced at every point is in accordance with the resistance at that point. Thus the sum of heat yielded is in exact proportion to the sum of resistances, and these, in their turn, to the sum of polarising force, or of energy yielded by the chemical action. This energy, therefore, produces the same heating effect whether set free in the cell or in the circuit. And necessarily the greatest production of heat is at the point of greatest resistance to polarisation, this being found, when the poles are not in contact, in the dielectric medium separating them.

(To be continued.)

IV. SHAM EMPLOYERS.

By J. Hepburn Davidson.

"A goodly apple rotten at the heart;
O! what a goodly outside falsehood hath!"
Merchant of Venice.

MANY employers are misjudged; many wronged by the vituperative tongue of their *employés*, and often condemned by the too open ear of the outside world; but there is still a heavy percentage of employers in general who richly deserve censure, and amongst those who do so no one is more prominent than our every-day business companion, the *Sham Employer*. Let us watch some luminary of the commercial world in his office. The bland courtesy which he displays to outsiders is exchanged for grim sternness. He tyrannises over his clerks, and especially bullies the one who receives the lowest pay. This unfortunate being, often subject—in accordance to agreement—to dismissal at a moment's notice "for gross misbehaviour," must beware even of a sorrowful look, which his lordship, the "Sham," might construe as a frown of disrespect. With managers or especial assistants, who have often a superior knowledge of the business, and by whose well-worked brains he earns his wealth (though he has not the grace to own it), the "Sham" plays a different game. He entices or forces some subordinate to play the spy upon their doings and sayings, in the hope of discovering some error, oversight, or transgression: for, strange to say, some employers do not like a servant against whom no charge can be brought.

Many employers are spoilt by lending a too credulous ear to the false tales of inferior servants, thereby losing the services of one who thoroughly knows his duty. A case happened in Scotland which is worthy of notice:—A certain gentleman advertised for a chemist and manager able to erect plant, &c., which was duly replied to by a friend. He entered upon his duties, and had erected nearly three-fourths of his plant, manufacturing at the same time with the first portion of the structure a sufficient quantity of goods to cover expenses. Suddenly an aspect of dissatisfaction came over his employer, and at the same time he noticed in his foreman an equivalent rise of confidence amounting to impertinence. This foreman was engaged not that he knew

his work, but for his "general good character and veracity." It is a general custom of some Sham Employers to give good credentials to nuisances that they wish to rid themselves of. At last the crisis came, through the utter defiance of his workman to obey him in regulating a furnace heat, who, after a little parley in words, put on his coat and walked out of the place, after throwing a threatening look behind, as much as to say "I'll do for you." Next morning the foreman in question quietly donned his best suit of clothes, and proceeded to meet his master at the railway station. Having met him, and having got over his employer's astonishment at seeing him off duty (as that species of men generally impress upon their masters the notion that their services are indispensable), he explained the reason of his absence in the most exaggerated terms, at the same time making personal allusions as to my friend's character, which was blameless. Our poor credulous Sham hired a cab, and at the same time lowered his dignity by driving a Judas with him to the Works. On arriving he at once charged against my friend the allegations that his subordinate had loaded him with, and would not even listen to the manager's denial of the charges, but in a patronising air told him to behave better in future, and ordered the foreman to take his place as usual, thus reinstating scandal and hypocrisy, and countermanding his manager's wishes. What man of education, and especially one who knew his profession thoroughly, and who was lining his employer's pockets with money, could bear this? You may act the philosopher to a certain extent, but at the moment injured innocence is predominant in man's feelings. The result was that our over-credulous and biassed employer lost a valuable help. He also lost heavily, as few were qualified to finish the plant or work it from where it was left, and those few would not engage with him, as they were established elsewhere and the positions they held were good. Other managers were tried, but the specialities to be manufactured were a puzzle to them, and he had to give up the idea of that manufacture and pursue a less lucrative branch, much to his own chagrin and to the amusement of others. I decidedly call this type of employer "Sham," from his over-credulous ear to an under workman, ignoring his manager's word, and listening to the preliminary scandal and stories of the workman, whilst at the same time acting with a fair hypocritical demeanour to his manager till the final crisis came.

Another one of the type is the "Sham" who would force untrue results from his servant, and if not complied with

instant dismissal is the result. In this case the incident happened in a large manure works employing a staff of three chemists. The head of the laboratory was a conscientious, scrupulous, and accomplished chemist, a neat manipulator who very seldom erred in his results. A sample of coprolites was sent up to the laboratory for analysis, with orders for his immediate attendance in his employer's room. The employer, in the course of general business conversation, actually wished his principal chemist—offering him a bribe—to report the phosphate of lime contained in the sample 3 per cent lower (phosphates were then at a high price per unit): naturally his *employé* was staggered, and, loving his professional repute and integrity better than his situation, took the wise course, and left. He threatened to expose his "Sham" master, but the other only laughed and remarked the utter absence of witnesses, and also threatened in return to raise an action for defamation of character. Another feat of the same gentleman was to order a mixed manure of a certain strength from his foreman (this foreman had been several years in his employ, and had the confidence of all around him), and afterwards to bribe workmen overnight to mix the heap with gypsum and sand, so as to depreciate the value. After the manure went through the process of chemical analysis, it was found wanting, and the consequence was the poor foreman had to leave with no recommendation for another situation. It turned out afterwards that a workman and he had words, the "Sham" having wished him to do some dishonest trick which his servant would not accede to, and his removal was effected by foul means, as described above.

All the above statements are well authenticated, and many readers will be horrified at the amount of subtle hypocrisy and craft exercised by dishonest employers. Better serve a stern, tyrannical, honest master, who exacts duty to the utmost, rather than a sneaking and scheming employer whose selfishness has deprived him of all moral feeling.

Some districts have more refined masters than others, and it is often found, though peculiarly inconsistent, that an uneducated master, rough and ready, is the most straightforward and honest. It may be that his very ignorance keeps down that refined diplomatic cunning which helps a man, dishonestly inclined, to injure another, and yet hold up his head in society.

In large chemical works, where the laboratory should be well stocked with apparatus and reagents of the highest attainable purity, so that the chemist may have a fair chance

to bring out good results, these are often refused by the tyrannical "Sham," who with an unblushing face in the commercial world boasts of the purity of his manufactures, and praises his chemist for the great ingenuity and skill he has displayed in the erection of most impossible plant; at the same time the poor chemist is plodding away with his make-shift apparatus, and struggling hard with his impure reagents to get his results correct, as his percentage is generally high above the mark. Let us take a glimpse at his laboratory:—On first entrance the place appears to be a ricketty hovel of a few panels nailed together. Along these panels a few shelves are set, and placed on these shelves we see a series of straggling bottles, of divers sizes, containing the reagents already mentioned, which, when pure, are the analyst's principal weapon. Near, a little dirty window, which it is impossible to see through; but if it were possible the look-out would not charm your fancy, as a series of not over-firm pillars sustaining the vitriol chambers meet the view, and it is not at all pleasant to reflect that through the penuriousness of an employer a few score tons of vitriol and lead may at any time pay respects to your cranium, when the only solace awarded would be a word of posthumous pity from the local press. The balance before us when new might have weighed the tenth of a grain, but now in its old age persistently refuses to turn clearly with one grain. Our harassed chemist is sitting, trying to make a quick and *accurate* weighing of a soda-ash, the results of which must be forwarded to head-quarters in ten minutes, and must exactly agree with the report of another independent analyst whose laboratory contains every modern appliance, whose balance will turn to a nicety with the five-hundredth part of a grain, and whose standard solutions are made up with the purest acids, while our hard-working technical chemist is begrudged his pure reagents when they have to be bought, and his standard acid must be made up of the crude pyrites-product manufactured in the Works, and contaminated with iron and arsenic. His weighing is scarcely effected when a Mercury from the office enters, and states that the "Guv'nor 's in a fearful fury, and says that he 's taking all day about that analysis." Our analyst was perspiring before, but now his nervous excitement predominates, as he knows what his master is. He now proceeds to titrate, and, having finished doing so, takes his reading and calculates, and finds to his horror that he is 2 per cent too low. What is to be done? There is no time for repetition. During his dilemma his employer enters, and uses language which dare not be

described, at the same time acquainting him with the fact that if the report was not forwarded by fifteen minutes, and that to coincide with the other analyst, he would be subject to dismissal as incompetent. I may scarcely add that the poor trodden-down chemist was dismissed for the tyranny of an ignorant rule-of-thumb alkali manufacturer, who would not scruple to violate the day on which we all of us should rest, by working him as hard as on any week-day. Many a scientific man is rendered careless in his professional habits in the laboratory by such employers. He gets into a slip-shod habit of performing his analysis, and learns the dishonest method of making up his results, commonly called "cooking" by the profession. A systematic "cooker" is generally well versed in the average percentage of the various samples he gets, and makes up his book accordingly, so that it can bear inspection by one of his profession, leaving alone a rule-of-thumb master who duly appreciates his smartness in executing his analyses.

There are other very high class "Sham Employers" commonly met with in the first laboratories. The principals of these abodes of learning are outwardly perfection, but inwardly "a goodly apple rotten at the core."

Sham Employers are so plentiful that it is dangerous to engage in a situation. The only apparent remedy is for the *employé* to make arrangements that references are mutually interchanged, but the danger even then is that one "Sham" will refer to another, and his servants are dumb. Strict enquiry through other firms of high standing may throw a ray of light; but the best advice is to try the place, do your duty, and care for no man.

The Institute of Chemistry was to have abolished the evils above mentioned. Has it even taken a step in that direction?

V. SANITARY REFORM AND ITS VAGARIES: THE SANITARY INSTITUTE OF GREAT BRITAIN.

IT being, apparently, an established fact that these islands are sadly under-peopled,—that we have food without eaters, work without workers, and space without occupants,—we are as a nation greatly exercised on the question of diminishing the death-rate. We have accordingly a movement among us known as "Sanitary Reform"—a movement boasting its statesmen ranging in type from the Conservative enunciator of "*Sanitas sanitatum et omnia sanitas*" to the Whig *doctrinaire;* its *savans* and—let it be whispered in confidence—its charlatans also. The latter are chiefly to be congratulated. Science is to them not "the high, the heavenly goddess," but the sturdy mule on which they ride to fame and fortune.

Leaving these worthies for a time, let us ask what our speeches and our lectures at the Sanitary Institute, our Commissions, and our letters to the "Times" have really brought about? We know little more concerning either the treatment or the prevention of cholera than when it first invaded us. Medical authorities of repute are beginning to doubt whether the immunity from Oriental plague which we have enjoyed for the last two centuries is due to our own skill and wisdom or to circumstances equally beyond our knowledge and our control. The very disease which we boasted of having overcome has recruited its strength, and returns upon us every few years in epidemic proportions, revealing the following curious case:—an evil increasing *pari passu*—if not faster—with its remedy. The mountains have certainly not brought forth a Megatherium.

But there is in Sanitary Reform and its upholders a certain degree of inconsistency, often overlooked. War is waged against "zymotic disease" only. Fevers, cholera, small-pox, diphtheria, and their allies are to be rooted out by any and every means. But does any Sanitary Reformer ever class among preventible diseases that long train of neuroses—affections of the nervous system—which culminate in madness, paralysis, apoplexy? Does any Institute ask earnestly whether it be true that such ailments are increasing

rapidly among us, what can be the cause of such an increase, and if nothing can be done to stay the growing evil? We do not in the least seek to under-rate the importance of the latest phase of Sanitary Reform, the agitation for a smokeless and fog-free atmosphere in our cities. But for one person killed outright, or enfeebled by a foggy atmosphere, we believe that many are cut off or debilitated by nervous disease, the offspring of worry and anxiety. It seems to us almost farcical to see persons connected with the system of competitive examination—and therefore promoters of worry—anxious about such a minor evil as a stove which does not consume its own smoke. The victims of fog are to a great extent elderly persons, whose part in the world is already played. Those who fall a sacrifice to examinationism—and they include a serious proportion of the successful as well as of the unsuccessful candidates—are young men who, had their strength not been exhausted by hurried work under the pressure of anxiety, might have done a good life-work, and become the fathers of vigorous descendants. By the wonderful scheme that we have devised they are reduced to shadows; "mox daturi progeniem deteriorem."

But even within the limited sphere of action which official sanitary reform has chalked out for itself,—to wit, the purification of the soil and the waters from animal, and especially human, excreta,—there is a want of broad views, of candour, of a disposition to recognise and to acknowledge the truth. All differences of climate, soil, situation, &c., notwithstanding, the work must be done in one way only—irrigation. Our true *doctrinaire* sanitary reformer seems to wear a kind of mental spectacles which enable him to ignore or to misconstrue facts in the most singular manner. Some years ago an expert, of irrigationist proclivities, came over to England from abroad, and inspected the principal systems of dealing with sewage. At the Knostrop Works, Leeds, looking through the spectacles aforesaid, he saw "six iron tanks." Men not provided with such optical instruments can see merely *twelve tanks of masonry*, and none of iron! That the writer's report on less palpable matters was not unimpeachable may well be taken for granted.

Again, we find it asserted, by men who ought to know better, that though earthy and metallic salts may precipitate the *suspended* impurities in water, they have no action upon the *dissolved* impurities. This statement has been made, among others, by a gentleman—neither physician, chemist, nor engineer—who had been "cramming" in order to read

a paper before a body which by its rules allows no man to bring before it matter not (in part, at least) the fruits of his own personal experience. This statement having been repeated in a morning paper, a chemist and physicist, of world-wide celebrity, wrote to ask upon what evidence so extraordinary a dictum was based? *His letter was refused insertion.*

As to the truth or falsehood of the assertion, let us remember that a very great part of the arts of dyeing and colour-making depends on the property of precipitating dissolved—not suspended—organic matter, possessed by metallic salts. Or, turning to a direct experiment, we would recommend all whom it may concern to take a sample of ordinary town-sewage, and filter it through a double-fold of the finest Swedish paper, so as to remove all suspended matter. If the bright, clear liquid which runs through is then mixed with a neutral solution of a salt, say of aluminium, it speedily becomes cloudy, and deposits a greyish precipitate. This deposit, if examined, is found to consist of nitrogenous organic matter along with a basic salt of aluminium. Comparative analyses of the water, before and after precipitation, corroborate the result, showing that the proportion of organic nitrogen is seriously lessened by the treatment with, *e.g.*, aluminium chloride. We wish that the zealots of irrigationism would condescend to perform this simple experiment, and lay to heart its lessons.

We turn to the late Royal Commission on the Pollution of Rivers, which lies sadly open to the charge of having merely sought for evidence in support of a foregone conclusion, carefully declining to see any facts of different tendency. It might be pointed out as very strange that this Commission should have consisted of one chemist, two engineers, and not a single physician. We should have formed it of two physicians and one chemist. But, passing over this peculiarity of constitution, let us ask what has become of the *last* report issued by the *first* Commission, of which Mr. Way was a member? It is known to have been drawn up, at least as a rough draft, but it has never been issued to the public, nor has its non-appearance ever been accounted for. Such an occurrence conveys to our mind a very unpleasant impression.

Among the strange, or rather the inexplicable, assertions made by the second Rivers' Pollution Commission is the statement that a certain precipitation process could not be worked without causing a nuisance in the neighbourhood. It is no exaggeration to say that it would be easy to find a

thousand utterly disinterested witnesses—medical men, chemists, engineers, municipal authorities, &c.—who have visited and carefully inspected places where the process in question was being worked, and would declare the above charge utterly unfounded. It has, indeed, been formally denied in the official report drawn up by Mr. Keats, chemist to the Metropolitan Board of Works, by the Sewage Committee of the Leeds Corporation, &c.; yet the slander—we can use no milder term—is still circulated as an argument against *every* chemical method of treating sewage, and its authors have never had the honesty to come and examine whether an observation, *perhaps* made under some peculiarly unfavourable circumstances, in the very infancy of sewage precipitation, holds good at the present day. Again, it was said that the effluent after treatment contained more ammoniacal salts than the original sewage. This was at the time perfectly true, and the Commissioners well knew wherefore, —that is, because ammonia alum was injudiciously used as a precipitating agent. Mentioning the fact they ought, in common candour, to have mentioned the cause, and to have admitted that such cause has been removed, and that the charge falls to the ground. So far from this, a former *attaché* to the Commission has repeated the statement within the last few years, though if at all acquainted with the history of the sewage-question he must know that in the process in question ammonia alum has long ago been laid aside in favour of cake alum (aluminium trisulphate).

Turning to another quarter: a precipitating process was worked experimentally in 1872, under close inspection. The chemical report as to the purification of the water and the freedom from nuisance was most favourable. The expense of working the process was made out to be ruinously heavy; but by what means? Coal was in the year 1872 at a fancy price, and the process was debited not with the ordinary cost of fuel, but with this special figure. Sulphate of alumina commanded then double its present price. More than that, in their anxiety to make the cost of the operations appear prohibitive, the authorities in charge insisted on debiting the process with the salary of a chemist who went down daily during the three months' trial to draw a check-sample of effluent water on behalf of the patentees! Yet in spite of the introduction of these and other palpably anomalous items, the document in question is still paraded by irrigationists as a proof of the prohibitive cost of chemical methods of sewage-purification.

But we must approach our more immediate question.

Before us lies a paper bearing the heading "Proceedings of the Sanitary Institute of Great Britain, June 21, 1881. Abstract—The Present State of the Sewage Question. By Prof. W. H. Corfield, M.A., M.D." In this document we find the words—"The *various chemical processes* for the treatment of sewage were *passed in review*, and *all* shown to be *quite* inadequate to cope with the difficulty." Now we have the authority of gentlemen present for declaring that *not even an attempt* was made to pass the chemical processes in review and to show their inadequacy. The subject was discreetly avoided. How, then, must the unqualified statement just quoted be characterised?

Prof. Corfield did indeed mention as untrustworthy—we believe a stronger term was used—the analysis of an effluent water obtained by a chemical process. Whence the effluent in question had been obtained, or by what process produced, the lecturer did not think it necessary to mention, but the figures quoted were identical with those obtained by Prof. J. A. Wanklyn on analysing a sample of effluent produced at Aylesbury by the process of the Native Guano Company, April 26th, 1880. The numbers were—Grains of chlorine per gallon, 3·2; free ammonia, per million, 0·05 part; albumenoid ammonia, 0·30, or only one-tenth part the proportions considered as admissible by the Rivers' Pollution Commissioners in their celebrated "Recommendations."

We learn, and we understand it could be judicially proved if necessary, that the samples operated upon by Prof. Wanklyn were taken by a Buckinghamshire sanitary inspector, without any warning given to the Native Guano Company or their officials. It seems to us that an attempt to discredit the work of a chemist of Prof. Wanklyn's known eminence, on mere conjecture, is, to say the least, highly indiscreet.

We must also remind Prof. Corfield that Prof. Wanklyn's analysis, whilst totally uncontradicted, is substantially confirmed by those of independent authorities. Dr. R. Angus Smith, in an official report, gives the Aylesbury effluent water a very high character. Surely, therefore, it may be contended that the lecturer's zeal had outrun his discretion. Judicious chemists are in the present day very cautious as to pronouncing on "chemical impossibilities."

Just as the defects of sewage precipitation were magnified, so those of irrigation were overlooked or understated. No one can deny that the effects of irrigation are to reduce the temperature of the soil, and thereby, in our climate, to retard the crops. This result is complained of at the

boasted sewage-farm of Gennevilliers, near Paris. Here there are produced great store of vegetables, strawberries, &c., but they come in late, when such articles have become a drug in the market. "Crops of all kinds had been grown by means of it (sewage irrigation)." Yet the committee appointed by the Local Government Board in 1876 are very far from recommending it for "crops of all kinds." For white crops it must be applied with great caution, even in the driest weather. Potatoes are not benefitted by it; with turnips the results are doubtful. The success has been greatest with rye-grass and mangolds. But it is very questionable whether rye-grass would be profitably saleable if produced on the large scale. As to mangolds we would suggest the following test:—Take a ton of mangolds grown by ordinary methods of cultivation, and another ton produced by sewage irrigation. Store them up separately in pits side by side, and examine after a few weeks which lot is the sounder. From cases which have come to our knowledge we will venture to say that the sewage-grown roots will be found largely rotten.

Here we are brought to the experiments of Dr. Smee, jun., which have never been refuted, and which *doctrinaire* sanitarians meet with the "conspiracy of silence." It was found that the juice of sewage-grass was chemically different from the juice of normal grass, and entered more rapidly into decomposition. It was observed that the milk of cows fed on sewage grass putrefied earlier and became much more offensive than the milk of cows fed on animal grass. Irrigationists dare not repeat these experiments and wish them to be forgotten.

We fully concede that in climates where a dry season occurs, or where rainfall is small,—either absolutely or in proportion to evaporation,—irrigation may be, and often is, the one thing needful to convert a desert into a garden. In Britain, where injury from drought is rare, and where our crops suffer almost yearly from an excess of moisture, the case is very different.

At one time it was contended that manures in general might be advantageously applied to the land dissolved, or at least suspended, in water. Experience did not confirm the expectations held out, and this method of manuring land has fallen into disuse. Is not this a case in point?

As regards the results put forward by the Sewage Committee of the British Association, to which Prof. Corfield refers, we have not forgotten that the independent members of that Committee found themselves compelled to resign,

and that the report ultimately adopted was a mere one-sided affair. The said Committee is declared to have "shown"—a favourite word in the "abstract" before us—that "as great a percentage of the manurial constituents of fœcal matters had been utilised as was on an average utilised of the best commercial manure." Yet, unless our memory greatly deceives us, it has been admitted by Prof. Frankland that from one-half to two-thirds of the nitrogen present in sewage may pass away from an irrigation field in the effluent water! This is very poor utilisation.

The dangers from the proximity of sewage farms are by no means purely imaginary. Even in India, where lands are irrigated with plain river-water, and only when the state of the weather renders such irrigation needful, it has been found necessary to prevent irrigation from being carried on within a certain distance of villages and to interpose a screen of trees. Is it conceivable that irrigation as conducted in England, with sewage applied at all times instead of clear water, used only when needed, will be the safer? If the proximity of a polluted stream is dangerous, is the danger lessened by spreading out the polluted waters over a larger area? Sewage-fungus is by no means unknown in the water-courses issuing from irrigation farms. That disease-germs are not necessarily arrested or destroyed by passing water through a vast mass of earth is shown by the celebrated Swiss case described by Prof. Frankland, where a stream which soaked through a mountain conveyed disease from one valley to the next.

As regards the economy of irrigation it has not been found upon the whole very satisfactory. Blackburn, Tunbridge Wells, and other places have found a sewage-farm a somewhat costly luxury. The report issued by the Committee of the Local Government Board tells us duly, indeed, under each town, that the capital sunk in buying and laying out a sewage-farm will be paid off in so-and-so many years, and the rates will then be reduced by a certain figure in the pound. But this comforting assurance is built upon the somewhat doubtful assumption that a given number of cubic feet of soil can go on for ever disinfecting putrescent animal matter. It is highly probable that further outlay will be required for extensions.

We fail to see, judging from the "abstract," that Prof. Corfield has advanced anything which has not been asserted and re-asserted many times during the last ten years. He brings forward no new arguments against "precipitation," he overlooks the improvements which have been made in its

practical working, and which enable it to produce results very different from anything ever realised, *e.g.*, at Leamington. Nor is he apparently able to point out any advances in irrigation, or to show the way of avoiding its besetting difficulties.

To prevent all misunderstanding it may be well to state that the present writer, though he has had thirteen years' experience in different processes of sewage-treatment, has no interest in the success of any scheme, either as a shareholder, patentee, or official, and judges from the point of view of an independent outsider.

VI. ON THE PROBABLE EXTENSION OF SOLAR PHYSICS TO MATTERS AFFECTING THE AGRICULTURIST, MERCHANT, AND MAN OF SCIENCE.

By A. H. Swinton.

THE scientific mind has of late years been gradually awakened to the important fact that a certain class of natural phenomena, on which mankind are greatly dependent for the future development of their resources, are immediately controlled by, and in direct subjection to, the central luminary of our system. Terrestrial magnetism, electrical activity, periodical variations in temperature, periodicity of wind disturbance, and annual rainfall have especially occupied attention in this respect; and their various phases following the vacillation of the spots on the sun's photosphere have been observed, recorded, and finally drawn up in tables and diagrams. From these indices we may now glean that as the sun's atmosphere of light, every eleven years or so, wanes and again replenishes itself, so will cold and warm seasons, cyclones and rainfall, disturbances along the electric wire, oscillations of the compass, and other symptoms in our sky and soil that receive the solar rays, follow each other in due sequence. Medical science, too, has not been oblivious to these periods of solar energy and

weakness, and not a few attempts have been made to correlate them with seasons of famine, plague, cholera, and other epidemic visitations. But with all this scope of inquiry indicated or grappled with, many other phenomena which the record of the past would lead us to associate with certain meteorological conditions, and therefore with solar physics, remain for the most part the subject of vague conjecture and inane speculation. Chief among these come, perhaps, seismic phenomena, including earthquakes, volcanic emanations, and hot springs; and entomological phenomena, such as insect multiplication and migration. As regards the first class of disaster—despite a prevailing astronomical bias to the theory propounded by M. Cordier, of a subterranean tidal disturbance governed by the moon, which yet remains a question—much that is palpable can be adduced in favour of the influence of the sun, more especially perhaps in the frequent arrival of these manifestations simultaneously about the equinoxes, and their recurrence in periods more or less marked. As regards the second class of subordinate phenomena more can be affirmed, and taking the insect plagues best known in the commercial world—such as locusts, corn weevils, and wheat flies—it may be fairly stated that their periods of increase and diminution show fair promise of exact co-ordination with the solar registers.

If we take, for instance, the rocky mountain locust (*Caloptenus spretus*), the scourge of the Western States of America, we shall find that its permanent region and native breeding-ground lies within the northern temperate zone, between 37° and 53° N. lat. Its migrations have been, on the whole, in a southerly direction east and west of this region; and from an analysis of the table given in the "Annual Report of the United States Entomological Commission for the year 1877" (p. 113) it is distinctly evident that the periods of migration and increase of this species coincide with the sun-spot minima, while its years of decrease as closely harmonise with the maxima, in neither case there existing the difference of a year. We also know as regards the multiplication and migration of various locusts in the cereal districts on the borders of Europe and Asia, from long tradition as from recent more exact evidence, that droughts coming about the minimum period of the sun-spot cycles are their time of increase. The evidence, however, is here of a more general description, and not so precise as in the case of Prof. Riley's elaborate reports, it being well known that "locust" in itself is a very wide and indefinite word to an entomologist. However accepted as a term of common

parlance denoting an insect of the Orthopterous class, it will be found that various records of locust increase, about the year 1810, along a line extending from Egypt to India, are extant: 1823 was a year when they commenced their depredations in the Crimea; in 1833 they were destructive about Marseilles; 1845 and 1866 mark ever memorable years of havoc in Algeria; all and each of these epochs indicating, as will be seen by reference to Wolf's tables, the minimum period of sun-spots as now more or less established.

But it must not therefore be assumed that all destructive insects have the same period of multiplication; for while the corn weevils of our granaries, or certainly the more destructive sort which is imported from the marts of the South, have shown a tendency to increase about the minimum of spots, it has likewise in measure been ascertained that the wheat flies (*Diploxis*), of which there are said to be two varieties, affect in Germany a decennial period recurring towards the maximum years. This phenomenon, I think, is in some cases doubtless attributable to the circumstance of the greater uniformity in the solar action within the zone of the tropics, and in others to the circumstance that insects are variously adapted to conditions of climate.

If, however, species thus show a tendency to increase at different periods, and there be recurring times when certain families and certain individuals find themselves in the most congenial of circumstances for multiplication, there yet remain fixed epochs when a general move is witnessed in the mass of the insect fauna. These, north of about 45° N. lat., alternate rather than agree with the extremes of solar energy, so that the great European migrations, as obtained by tabulation of pamphlet notices, may be stated to have recurred regularly every eleven years since 1846, or at times when the sun-spot average has been 48·6 according to Mr. Norman Lockyer's method. The set of these migrations has been north and west, and in this direction rare butterflies, sphinx moths, and locusts, whose home has been traced to southern Asia and northern Africa, travel periodically; the inauguration of the occurrence being made known by their vanguard, so to speak, sweeping over the eastern shore of our island. That this track is not voluntary or chosen by instinct, as some have supposed, but due rather to a prevailing south-easterly direction of the wind, is a matter that now-a-days rests on a great amount of experience and observation.

It is likewise of interest to note, in connection with these flights, that although they traverse Europe between the

extremes of solar power, they often originate, and in cases can be traced to, a multiplication of species that has transpired during the minimum period. Especially is this probable with regard to the locust inroads which, pushing northwards, have found their chroniclers since the Christian era, and which, from unknown climatic or agrarian reasons, appear formerly to have been more formidable or more dreaded by an ignorant population. Another minor migration of European insects, much less marked, occurs in the same direction as the greater flights between the maxima and minima sun-spots, or when the index points at 38·5; I am, however, unable at the present moment to affirm whether this faunistic displacement should be attributed to the greater or less period of solar force.

I think I have here adduced sufficient to show the importance of solar physics, and to indicate their probable line of extension to a common ground where the man of merchandise may meet with his scientific brother, and learn the reason of his triumphs and losses, which, by being recorded, may eventually be remedied. But not alone the merchant; the astronomer likewise has somewhat to learn of the entomological record, for he will here find ready data which when elaborated will allow him to extend his solar cycles back into the Dark Ages which preceded Schwabe and Galileo, and in this wise:—Since on tabulation it will be seen, even at first sight, that ancient locust invasions in Europe have taken place at intervals that homogenise with those following modern migrations; and since the latter intervals come on at the maximum of spots, when the insects regularly disappear and the ravage ceases; few will doubt but what in collating the evidence of the havoc we are virtually pointing the sun-spot maxima. So from what has been before adduced the minimum periods may be similarly approximated and fixed, by correlation of the records of locust multiplication in localities situated on the edge of the northern tropic, as, for example, the districts of southern Europe and northern Africa, Persia, or China.

ANALYSES OF BOOKS.

The Cat. An Introduction to the Study of Back-boned Animals, especially Mammals. By ST. GEORGE MIVART, Ph.D., F.R.S. London: John Murray. 1881.

A LARGE handsome volume entitled "The Cat" will probably cause some little surprise to the ordinary reader as to what can be found to fill it. An animal which is as much an object of aversion to some as it is a pet and a friend to others would hardly be deemed worthy of being honoured with so splendid a monograph.

A careful inspection of the book itself will not only show how much valuable and interesting matter has been collected by the author, but will even suggest the thought that the volume, large as it is, might have been considerably extended without exhausting the subject.

The cat is throughout the work treated as the type of the Vertebrates in general and of Mammals in particular. The reason why the cat is selected will best be explained in the author's own words in the Preface :—"It might perhaps be expected that Man himself would be chosen as the type. But a fresh description of human anatomy is not required, and would be comparatively useless to those for whom this work is especially intended. For a satisfactory study of animals (or of plants) can only be carried on by their direct examination, the knowledge obtained from reading being supplemented by dissection. This, however, as regards man, can only be practised in medical schools. Moreover the human body is so large that its dissection is very laborious, and it is a task generally at first unpleasing to those who have no special reason for undertaking it. But this work is intended for persons who are interested in zoology, and especially in the zoology of beasts, birds, reptiles, and fishes, and not merely for those concerned in studies proper to the medical profession. The problem has been to select as a type for examination and comparison an animal easily obtained, and of convenient size; one belonging to man's class, and one not so different from him in structure but that comparison between it and him (as to limbs and other larger portions of its frame) may readily suggest themselves to the student. Such an animal is the common cat. In it we have a convenient and readily accessible object for reference, while the advantages which would result from the selection of Man as a type will almost all be obtained without the disadvantages of that selection."

The introductory chapter is one that will be read with equal interest by student or general reader, and contains much to please and instruct.

The domestic cat is shown not to be the wild cat (*Felis catus*) tamed, but is supposed to be a descendant of the sacred cat of the Egyptians, possibly with an admixture of other blood, the parent of which was the *Felis maniculata*, a native of Northern Africa. The domestic cat was a rare and valuable creature in England during the Middle Ages, while the wild cat was extremely common. So great a value was set upon the animal towards the end of the sixth century that the laws of Wales, Switzerland, Saxony, and other European countries, imposed a heavy fine on cat-killers.

"The domestic cat is an animal so common and familiar that its utility is sometimes apt to be lost sight of. To realise its usefulness we must imagine ourselves in a land where no such animal is known, but where the annoying creatures upon which it preys shall have multiplied with the rapidity natural to them. The familiar tale of Whittington may serve to illustrate what would be the effect of its introduction into such a land. It has been calculated that a single cat may devour twenty mice in one day; but this of course is by no means the limit of its powers of destruction. Its effect in putting to flight the creature it pursues is again far in excess of its destructive energy. Were every cat in England simultaneously destroyed, the loss through the entailed increase of vermin would be enormous.

"But however much this animal may deserve our esteem, or win our admiration, by its shapely form and graceful movements, it certainly has very special claims on the attention of lovers of biological science. For in the first place its organisation, considered absolutely in itself, is one of singular perfection, and the adaptation of means to ends which it displays is truly admirable. If, however, we compare its organisation with that of other animals, we shall by so doing not only gain a better appreciation of its structural perfections, but also become acquainted with a variety of relations conveying useful lessons in anatomy, psychology, and zoology, and others referring to the past, the present, and even the future history of this planet."

So much for the author's own apology for his selection of the cat: the latter part of this very interesting chapter explains very fully the object and scope of the work.

The next eight chapters are devoted to an exhaustive account of the anatomy of the cat, occupying about one-half the volume: here we have evidence of the author's patient investigations, and his determination to aid to the utmost of his power those wishing to study the anatomy and histology of vertebrate animals. The writer has evidently seen and carefully examined everything that he describes, and this, undoubtedly the most valuable portion of the whole book, is unsparingly supplied with

all necessary illustrations, microscopical and anatomical. The tenth chapter is devoted to Embryology, and is quite as complete and exhaustive of the subject as the preceding treatise on Structure.

The Psychology of the Cat is treated of in a more extended sense than is usually applied to the term. In the author's words it denotes "the study of all the activities, both simultaneous and successive, which any living creature may exhibit," and does not merely signify mental states. With regard to the latter part of the subject, the "Cat-mind," a quotation will best explain the author's views:—

"We cannot of course, without becoming cats, perfectly understand the cat-mind. Yet common sense abundantly suffices to assure us that it really has certain affinities with our own. Indeed the cat seems to be a much more intelligent animal than is often supposed. That it has very distinct feelings of pleasure or pain, and keen special senses, will probably be disputed by no one. Its sense of touch is very delicate; its eyes are highly organised* and can serve it almost in the dark, and its hearing is extremely acute. It is also obvious that external and internal sensations, more or less similar to ours, by which we move instinctively from place to place, judge of distance and direction, and perceive resistance and pressure, must be possessed by the cat also. The ease and grace of motion in the cat, and its neat dexterity, are a common subject of praise." Much more in this chapter as to the high mental endowments of the cat, but, however interesting the subject, the temptation to quote further must be resisted: it is only to be regretted that additional information respecting cat-life has not appeared in an appendix.

Then follows a description of fifty species of living cats, and also a full account of fossil species, which appear to have been somewhat numerous. This chapter is liberally illustrated.

The chapter on the Cat's place in Nature, in which the author answers the question "What is a cat?" is a grand lesson in classification. After disposing, in a brief manner, of the differences between minerals and organised beings, the vegetable kingdom is separated from the animal. The lower animal king-

* Page 290, after describing the structure of the choroid and its lining with a layer of dark pigment cells, excepting at a part called the *tapetum*, "a roundish patch occupying most of the back of the inside of the choroid, and including within it the entrance of the optic nerve. It is this tapetum which gives the eyes of cats that luminous appearance in obscurity, by reflecting the light—a property which is supposed to assist their nocturnal vision."

"At my request Mr. Henry Power has been so kind as to examine the cat's eye with the ophthalmoscope, and says 'I owe you my thanks for directing my attention to one of the most beautiful things I have ever seen. Imagine a dense, yet luminous, velvetty blackness below, bounded by a nearly horizontal line, just above which is a pearly spot—the entrance of the optic nerve. This presents the usual vessels emerging from it. The disc is surrounded by a sapphire-blue zone of intense brilliancy, passing into metallic-green; and beyond this the tapetum shines out with glorious colours of pink and gold, with a shimmer of blue and green. It is really lovely.'"

doms are then dealt with, and the type (the cat) compared with each, and the agreements and differences carefully noted. No one could read carefully this earlier portion of the chapter without learning much concerning the whole of the Invertebrata. Fishes, Batrachia, reptiles, and birds are then compared with the cat: the same plan is still adhered to, but as the structure of the types described becomes more complex, so greater extension of the comparisons is more and more fully worked out; after each the matter is summed up in a useful tabular form; at last the Carnivora are reached, and here the differences are still more minutely defined. The verdict as to the cat's place in the animal kingdom can only be given in the author's own words:—

"The cats are then Carnivora *par excellence*, and they carry out the type of their order to its higest known and most perfectly harmonious expression. Spontaneous activity and sensitiveness are the special characteristics of animal life, and with both these powers cats are largely endowed. It may be objected, however, that the activities and sense perceptions of certain other beasts are, in their own various ways, as highly developed as are those of the *Felidæ*. It is certainly very true that it is only through the possession of perfectly-formed bones and muscles, of a delicate sense of hearing, or of far-reaching vision, that antelopes, hares, and such creatures, escape their carnivorous pursuers. But then they use their organisation only for *escape*. The organisation of the cat tribe may then be deemed superior, because it is not only excelled in itself, but because it is fitted to dominate the excellences of other beasts. Thus considered, the Carnivora would rank first amongst mammals, and the cats would rank first amongst the Carnivora. Man, however, is a mammal, and therefore to affirm this would be to affirm the inferiority of our own species. But man's superiority is mental; it resides in his intellect, not in his peculiarly formed great toe, hand, pelvis, or other corporeal peculiarity. Man is to be regarded in two lights—as a truly intellectual being, and as an animal with a certain organisation. Viewed in the first mode he stands quite apart from and outside the whole visible creation, and has simply no place whatever in any scheme of biological classification. Considered merely in his capacity as an animal, he has a very definite place in such a scheme, but it is by no means certain that his place is at the summit. Our powers of locomotion and sense perception are quite inferior to those of very many beasts; and though our brain is large, both absolutely and relatively, yet such are the variations in this respect, presented by animals of different groups, and by different animals of the same group, that the naturalist would be a bold one who should venture to affirm that a brain-classification of vertebrate animals—to say nothing of Invertebrata—would be a satisfactory one. The close bodily resemblance of the apes to man gives them no just claim to a rank above that of the Carnivora, since such

a claim only reposes on their bodily resemblance to ourselves. As to their intelligence, no evidence seems to be forthcoming that it is superior to that of the dog or the elephant, though their close likeness to ourselves gives to their tricks a deceptive appearance of rationality which we must always be careful adequately to discount if we would correctly estimate their real worth. It is therefore true that 'something may be said in favour of cats being the highest of mammals,' if man be considered merely in his animal capacity,—in which alone he can be brought into comparison with other organisms. But whether or not this eminence be allowed to the cat, there can be no question but that it is the most highly-developed type of carnivorous mammalian life—the most perfect embodiment of a 'beast of prey.' Such, then, is *certainly* the 'CAT'S PLACE IN NATURE.'"

The chapter on the Cat's "Hexicology" treats on the cat's relations to its surroundings, including physical conditions, geographical distribution, palæontology, and its relations to other living beings. Among cats' enemies the most troublesome are internal and external parasites: of these a goodly list are given. From their habit of feeding upon small animals, cats—as the owners of these pets well know—are particularly subject to the attacks of various Entozoa, and the ravages of these worms are common to the whole family.

The work concludes with a chapter on "The Pedigree and Origin of the Cat." An abstract of this important part of the book would be impossible, from the necessarily complex argument involved, and quotations here and there would do no justice to the author; let it suffice to say that the chapter is the finest in a splendid book. The writer's aim is to teach the truth, and not to support a view.

The book will take its place as one of the most valuable monographs yet produced.

Education, Scientific and Technical; or, How the Inductive Sciences are Taught, and How they Ought to be Taught. By ROBERT GALLOWAY, M.R.I.A., F.C.S. London: Trübner and Co.

FOREIGNERS often wonder by whom the examinational system of education rampant in England and in China is really defended. Our scientific discoverers—we need only mention the names of Huxley, Crookes, and Lankester—reject it with contempt; our most learned medical authorities denounce it as ruinous to the mental constitution not merely of the present, but of future generations, and pronounce the work of the Civil Service

Commissioners individually and racially destructive.* Actual experience—the hard logic of facts—shows how inferior we are in the quantity of first-rate intellectual work produced to the nation which discards emulation from its system of education, and which is in consequence of its thoroughness leaving us more and more behind, not merely in scientific research, but in the industrial arts. Who, then, are the infatuated individuals who still hug this delusion, compelling our schools and colleges, often against the better judgment of their principals, to proscribe all original thought? The answer is not far to seek. The mischief is due to politicians ever ready to sacrifice such a trifling matter as the intellectual life of the nation to the exigencies of party. To those who see a little farther and more clearly than a certain peer who recently defended competitive examination in public, the intellectual and physical degradation of a great part of our most promising young men seems far too heavy a price to pay for the suppression of "influence." To call examination a test of merit is ridiculous indeed, if we call to mind the cases placed on record within the last few years, of two men taking high honours respectively in chemistry and biology, though the chemist had never cleaned out a test-tube, and the biologist never looked through a microscope or determined a vegetable or animal species! But as this evil system is defended by men who, for our sins, are invested with power and influence, the work of Prof. Galloway is most opportune.

The author's object is, as he admits, not perfectly explained by the title which he has given his book. He begins with the fact, now generally admitted, that in higher education—whether of an abstract or of an industrial character—we are inferior to certain of our neighbours, especially Germany. It is not generally known in this country that for annual fees of about £15 a young man can obtain—*e.g.*, at the Polytechnic School of Aix-la-Chapelle—a technical training quite as good as that given in the Royal School of Mines, and more varied in its nature, according as the student seeks to qualify himself for the duties of a technological chemist, a mining, civil, or mechanical engineer, &c. It cannot fail to be known to all that Germans are continually flocking over to this country, and, in virtue of their superior education, supplanting the natives in almost every sphere where technical skill is required. Prof. Galloway asks, What is the reason for this inferiority? He shows that the defect does not lie, as is often said, in the mere want of funds. At the same time it may, perhaps, be thought that some of the money now expended in forcing elementary education upon those who do not want it might have been better applied in supplying higher education to those who are craving for it.

Neither are our shortcomings due, as the author shows on good

* *See* Journal of Science, 1881, p. 440.

evidence, to any inherent defects in the British and Irish mind. The causes, which are very complex, include the want of unity of design in our national institutions. We set two or three distinct bodies to do work which should be committed to one alone. He quotes from Dr. Weise the very just remark that with us "public instruction in general does not show a progress towards objects clearly recognised and defined. An extraordinary amount of power, time, and money is still wasted, from a want of plan and unity."

Another evil lies in our defective patent law, which renders it almost impossible for a poor man to secure a right of property in his ideas. We venture to say that the patent laws of the United States are in themselves a most powerful agency for the technical education of the American people.

Another cause of our deficiencies lies in our excessive attachment to literature as compared with physical science. We love words and dreams rather than things. We honour the orator, the novelist, the historian, the lawyer, the popular preacher, and the political agitator, far more than the inventor or the discoverer. And as men, of whatever mental calibre, cannot dispense with food, shelter, and clothing, it follows that a majority of our best minds gravitate towards the pursuits which are most honoured and best remunerated. Even in the present day, when efforts are being made to supply better facilities for scientific culture, the friends of literature, not yet satisfied with the lion's share of time, funds, and honours which they already possess, cry out for more. It seems that a college which shall confine itself to Science must not exist in England.

But if Prof. Galloway somewhat overlooks the exclusively literary tendencies of the average Englishman, he is the more explicit and satisfactory on the defects of our method of teaching. Our cardinal sin is the almost exclusive cultivation of verbal memory. This, again, is a necessary result of the system of prizes, certificates, and of competitive examination in all its phases. The teacher is, directly or indirectly, paid by "results." The more of his pupils "pass" such and such an examination the more will his reputation and his emoluments rise. Hence this "passing" becomes a primary object. As our author puts it, he selects those of his pupils who have a good verbal memory, joined, we may add, with glibness of speech and a considerable share of assurance, and to these he pays his almost exclusive attention. These select few he trains up not in a thorough knowledge of the subject, but in the art of answering questions upon it. What those questions are likely to be he ascertains approximately by collating the examination papers of former years, and by carefully studying the whims and the predilections of the examiner. He knows what theories the latter believes in, and whose text-books he prefers.

It is plain that by this process a kind of unnatural selection is

effected, most unfavourable to merit. For what class of pupils are best calculated to shine under this system? Let us take two lads, A and B. A has an inquisitive, thoughtful, suggestive mind. If a fact is laid before him he begins to ponder on its possible bearings. If a theory is brought forward he instinctively asks for its evidence. Meantime B, blessed with a retentive memory and a rapid power of reproduction, gobbles up all that is thrown before him, and is able to bring it forward again, wrapped in a torrent of verbiage, at a moment's notice. B passes, in virtue of his very shallowness and emptiness; A fails, in virtue of the very fertility of his mind and his inclination to follow out every idea to its consequences. Yet this is Earl Kimberley's "merit!"

Students are examined, not *e.g.* in chemistry, but in what Prof. Frankland or some other reputed authority has written upon chemistry. We hear, therefore, of men "reading" for a degree, not "working" for it. The difference is heaven-wide. Hence, as the author quotes from Dr. Playfair, we have "faithful disciples rather than independent thinkers. The diversity of teaching in different universities tends to mitigate this evil, but the uniformity of *a common system of examination vastly augments it.* When Government takes graduation in hand, and stamps our intellects, as it does its sovereigns, with one uniform die, the power at its disposal will be immense; but, as in France, the intellects will in time be crushed under the stroke, and then will not be worth the coining."

It might have been expected that Englishmen, once proud of their mental independence and individualism, would have resisted such a system to the death. But alas! we look on in apathy whilst a *parvenu* bureaucracy casts the intellects of our youth into one and the same mould, and positively restrains our teachers from throwing into their work individuality and intelligence.

If we look to the German universities we find, on the contrary, diversity. Each of the great laboratories presided over by Bunsen, Hofmann, Kolbe, Fresenius, &c., has its distinguishing features. In other sciences it is precisely the same. A student will, therefore, after passing a couple of terms at one university, proceed to another, where he may have the opportunity of working under Prof. X or Y, and of studying his science from a different point of view. "Here in England teachers can display no such independence of thought; they have to walk themselves and make their pupils walk in the paths of science the examiners walk in, although they may not approve of it."

In Germany emulation and competition are not made the pillars of education. The Director of the Barmen Technical School declared that "the principle of competition was almost entirely excluded from their system, as tending to foster a servile view of education, and to lead to spasmodic and exhausting efforts and feverish excitement, rather than to the healthy and harmonious development of the mental powers."

Prof. Galloway, in our opinion, rightly contends that prizes and medals ought to be abolished, and that the teacher should not be paid by "results" in the cant sense of the term,—that is, by the number of his pupils who "pass" some particular "standard." In one sense—though in a very different one—German professors are paid by results. The "results" are the number and the magnitude of the discoveries and investigations emanating from their laboratories, whether physical, chemical, or biological. In proportion as such discoveries rise in importance students flock to the university, and honours and emoluments fall to the lot of the professor.

We regret that we must here, from want of space, break off our examination of Prof. Galloway's ideas, the more as we have not yet touched upon one important phase of the subject—his method of teaching the physical sciences. To this question we hope to return at no distant opportunity. Meantime we can do no other than pronounce the work before us an able, a wise, a timely, and a patriotic production—in short, a book after our own heart.

The Occult World. By A. P. Sinnett. London: Trübner and Co.

Here is a book which will be in most quarters received with contemptuous incredulity, and flung aside after being made the subject of a few jokes. Such has not been our feeling. We are too bitterly conscious of the imperfect and fragmentary character of human knowledge,—too deeply convinced how frail are the foundations of many of our fashionable creeds,—to reject light coming from what quarter soever. We therefore opened this book in the hope of finding something definite and tangible. Our curiosity was raised by such passages as the following:—"Modern physical science has been groping for centuries blindly after knowledge which occult philosophy has enjoyed in full measure all the while." "Achievements far more admirable than any yet standing to the credit of modern science." "Secluded Orientals may understand more about electricity than Faraday, more about physics than Tyndall." "During a career which has carried occultism in the domain of physical science far beyond the point we have reached, physical science has merely been an object of secondary importance," &c. Physical science being for us an object of primary importance,—in fact, *the* object,—our interest and curiosity were most strongly excited, and we sought eagerly for some proposition, whether the statement of a fact or of a law of Nature, which should be capable of verification. Can no crumb be vouchsafed us of this alleged knowledge so far beyond our own?

There is, we are told, a force in Nature known in Sanscrit writings as *akaz*, said to be "as much more potent, subtle, and extraordinary an agent than electricity, as electricity is superior in subtlety and variegated efficiency to steam." But about the attributes and the laws of this force, and the manner in which it may be observed, we are told nothing! We find complaints of "the intricate suspicions with which the European observer approaches the consideration of the marvellous in its simplest forms. But, setting aside the idea of the "marvellous" as scarcely scientific, how can we, without the sceptical, critical spirit, separate truth from delusion? The man of science, if he finds in the quiet of his own laboratory, and without the presence of any other person, indications of a new element or of a new force, multiplies tests, and checks, lest he should unwittingly deceive himself, and the more widely the novelty stands apart from what has been previously known the more rigid is the scrutiny. Hence the treatment which the advocates of occult science receive, and which seems to them "tiresome and stupid," has in it nothing exceptional. But, indeed, judging from a letter of an adept, occult science is not something which can be superadded to our open modern science as a continuation or an improvement. We read—"You do not seem to realise the tremendous difficulties in the way of imparting even the rudiments of *our* science to those who have been trained in the familiar methods of yours. The more you have of the one the less capable you are of comprehending the other." Again, "Exact experimental science has nothing to do with morality, virtue, philanthropy, and therefore can make no claim upon our help until it blends itself with metaphysics." Hence to us men of the laboratory, the observatory, the zoological station, or the museum, this occult science has nothing to offer!

A number of occurrences are described in proof of the mighty, and to us unaccountable, powers possessed by the adepts. We do not presume to deny that these are records of actual facts; but we must be permitted to say that evidence very far more convincing, and incapable of imitation by jugglery, might easily have been produced. If such men as Koot Hoomi Lal Singh were to announce some fact not at present known, such as the existence of an undiscovered planet, the presence of an as yet unrecognised element in some particular geological formation, &c., the verification of such predictions would silence all gainsayers.

It is here stated:—"The philosophical and transcendental notion of the mediæval theosophists that the final progress of human labour, aided by the incessant discoveries of man, must one day culminate in a process which shall result in the evolution of nutritious food out of inorganic matter, is unthinkable for men of science." Strange assertion! The synthesis of sugar, starch, fats, albumen, is not merely thinkable, but is being eagerly sought for,

We close this book not without regret. We are, we trust, open to any new truth. But unless the disciples of occultism can speak more intelligibly they had surely better keep silence altogether, and not raise expectations which they have always such a wealth of good reasons for not gratifying. But how profound soever may be the Science of the Oriental adepts, what would be the use of their communicating it to Englishmen? We should simply incorporate it in our different grades of cramming, and examine unfortunate young men in it. So long as we put our knowledge to this use it may fairly be contended that the less we have the better.

Songs and Sonnets of Spring-time. By CONSTANCE C. W. NADEN. London: C. Kegan Paul and Co.

HERE surely is a twofold marvel—that a poet should care for notice in our highly prosaic pages, and that the subject-matter of some at least of the poems in question is such that we may examine them without inconsistency on our own part. It must not by any means be supposed that all men of science regard poetry as "mere sensuous caterwawling." The thoughtful ethnologist sees in it an index of national life: where it ceases to appear, or, if appearing, to be appreciated,—as in modern England, or in Greece of the post-Periclean epoch, or in Rome after the days of the twelve Cæsars,—such national life is fading. Where it never has appeared the nation, as a whole, has not yet begun to live, in the higher sense of the word, and perhaps never may do.

The "Songs and Sonnets" before us have an especial interest as dealing, to no small extent, with certain scientific and philosophic subjects. Let us not be mistaken: we have here no elementary treatise or handbook done into verse. Miss Naden pictures the man of science clinging with devotion to his chosen pursuits, and yet ever and anon regretting to what an extent his researches cut him off from the sympathy of his kind. Thus, in "The Astronomer," she writes:—

> "Bright hieroglyphs I read in Heaven's book;
> But oft, with eyes too dim for these,
> In half-regretful ignorance I look
> On common fields and trees.
>
> Scant fare for wife and child the fisher gains
> From yon broad belt of lucent grey;
> Rude peasants till those green and golden plains;
> Am I more wise than they?
>
> Oh, far less glad! And yet could I descend
> And breathe the lowland air again,
> How should I find a brother and a friend
> 'Mid earth-contented men?"

What man, devoted to scientific research, does not find such thoughts passing at times through his mind? But yet, with the authoress, he can truly say,—

"Now has the breath of God my being thrilled,
Within, around, His word I hear;
For all the universe my heart is filled
With love that casts out fear.

My sight, love-strengthened, Time and Space controls;
No more are Force and Will at strife;
Beyond the scene I pass; around me rolls
Infinite circled life."

Somewhat similar, but less calmly hopeful, are the meditations of the "Alchemist"—thoughts such as may have occurred to a Paracelsus, or a Raymond Lully in the evening of his days:—

"In lonely toil my manhood has been spent,
Spurning all ties of home, all joyance free;
And now my heart is sick, my frame is bent,
And I would sleep, but rest is not for me.

I conquer still, though strength may not be mine
To drink the cup my dying hand prepares;
My life, but not my triumph, I resign,
For all mankind shall be my deathless heirs.

I care not who the victor's crown may wear,
I care not though my bones neglected lie;
This is my latest, this my only prayer—
Come life, come death, let not my wisdom die."

Surely these words are not unworthy of the grand old sages who first smoothed the roads along which we are travelling!

From "Light at Eventide" we quote the reflection:—

"Evil has brought good, but good in turn
Brings evil forth, and painfully we learn
The rich resulting harmony of life."

Throughout these poems we find, indeed, full evidence that the writer is familiar with the highest conceptions of modern philosophy. She embodies its doubts, its struggles, its aspirations,—beautiful indeed, but with that autumnal, or sunset-like, kind of loveliness which pervades all modern English poetry.

We are particularly struck with a short piece, "Das Ideal," written in German, which would not have disgraced Heinrich Heine, and from which we quote the following lines admirably embodying the longings of the thinker:—

"Zerreissen will ich die geträumten Schleier
Des Stoffs, des Raums, der Zeit,
Und mich ergiessen, frei und immer freier
In die Unendlichkeit."

"The Lady Doctor" is a comical portrait of one of the characteristic phenomena of the present day. "Love *versus* Learning" embodies the meditations of a maiden who had planned "to be a philosopher's bride," and with that view had become engaged to an Oxford examinee, and discovers his shallowness.

> "He's mastered the usual knowledge,
> And says it's a terrible bore;
> He formed his opinions at college,
> Then why should he think any more."

Our readers, if inclined to blame us for noticing this little collection of poems, are recommended to read it for themselves, when they will doubless give us a full pardon.

Australian Aborigines. The Languages and Customs of several Tribes of Aborigines in the Western District of Victoria, Australia. By JAMES DAWSON. Melbourne, Sydney, and Australia: George Robertson.

CONSIDERABLE attention is now being paid in all parts of Australia to ethnological research, under the impression that if the opportunity is not at once taken the gradual decline of the native tribes will soon make the undertaking impossible. Even now it is difficult to sift the original ideas and beliefs of the "blackfellows" from such as have been acquired from the settlers. The author forms a very favourable opinion of the natives in many respects. He praises, *e.g.*, their sanitary observances. They invariably bury all excrement in the earth at some distance from their dwellings, though their reason for this wise practice is based on superstition. It is a curious fact that a black louse which formerly infested the natives has disappeared, and the white louse of Europe has taken its place. The common flea was also not indigenous,—a fact to be weighed by those who maintain that such vermin have some important function to fulfil.

The cleanliness of the natives must be taken in a relative sense, if we remember that they coat their bodies with a paste made of grease and red clay. As far as the tribes in question are concerned, we learn that "the figures of human beings, animals, and things now drawn by the natives, and represented as original in works on the aborigines of Victoria, were unknown to the tribes treated of, and are considered by them as of recent introduction by Europeans."

Concerning food they have a mixture of notions, some sensible and others exceedingly foolish. They are said "never to touch

putrid flesh, except that of the whale," and yet a few lines lower down we read "they have no objection to eat tainted flesh or fish: if it is too far gone it is roasted, to expel the unpleasant flavour." It seems to us hard to draw a definite line between what is putrid and what is tainted. Fish that have been exposed to the rays of the moon are rejected as poisonous. A similar notion prevails in many other parts of the world, and it is maintained that such fish or other animal matter is particularly liable to putrefaction.

The explanation has been suggested that dew is deposited most abundantly in moonlight nights when radiation from the earth's surface is unchecked. Now if dew consists of moisture condensed in the lower regions of the atmosphere, it may not improbably be fraught with the spores and germs of microbia, including some capable of bringing on malignant fermentations. The prohibition of certain classes of food to persons of one sex, or at some particular time of life, does not seem capable of a rational explanation.

Their exceedingly complicated marriage laws are said to have been devised to prevent the intermarriage of blood relations. Any physical advantage hence derived is probably more than counterbalanced by the polygamy permitted to the chiefs, who, though mostly old men, are permitted to monopolise the finest young women.

A kind of python, about 10 feet in length, is met with in Victoria, and sometimes attack human beings. They are much dreaded by the natives. The carpet- or tiger-snake is also said, contrary to the general law laid bown by Waterton, to attack men without provocation.

Mention is made here of the "bunyip," a nondescript animal inhabiting water-holes, concerning which the natives relate sensational stories. It is said to carry off a man in its mouth. Making all proper deduction for exaggerations, it is possible that the "bunyip" may be some Saurian which has not fallen into the hands of any naturalist. The descriptions given do not, however, suit any kind of crocodile, as this animal is said to have a long neck and to have the power of erecting its head.

Native tradition speaks of a huge bird, probably the moa of New Zealand.

A species of eagle, not named, occasionally attacks young children, and is much dreaded by the natives.

Traditions of earthquakes, and even of volcanic action, still survive.

The bulk of the work before us is devoted to the languages of the West Victorian tribes, and contains extensive vocabularies arranged in parallel columns.

CORRESPONDENCE.

*** The Editor does not hold himself responsible for statements of facts or opinions expressed in Correspondence, or in Articles bearing the signature of their respective authors.

AËRIAL NAVIGATION.

To the Editor of the Journal of Science.

SIR,—As the writer of the greater part of the article on "Aërial Navigation" in "Progress of Science" for May, I trust you will pardon my referring to the criticism thereon in the July number of "Journal of Science."

The objection therein made to such "an enterprise" is "that it will give a fearful advantage to aggressive war."

Believing as I do that the ultimate result of aërial navigation will be to practically abolish war, which I cordially detest as being inimical to progress, I regret that the mass of arguments for or against any views on the subject must be merely speculative so long as the chief problems of aëronautics are publicly unsolved. I can therefore only tender opinions in place of arguments.

The first I quote are those of an eloquent writer in "Scribner's Monthly":—

". . . The great peoples of Christendom soon will arrive at a common understanding; the Congress of Nations no longer will be an ideal scheme, but a necessity, maintaining order among its constituents, and exercising supervision over the ruder, less civilised portions of the globe. . . . *War between enlightened nations soon will be unknown.* Men will see 'the heavens fill with commerce,' but after a few destructive experiments there will rain no—

'ghastly dew
From the nations' airy navies grappling in the central blue.

Troops, aërial squadrons, death-dealing armaments will be maintained only for police surveillance over barbarous races, and for instantly enforcing the judicial decrees of the world's International Court of Appeal. . . . Material progress determines the intellectual and spiritual progress of the human race. Its

true perfection must follow this ultimate conquest of Nature, and can be reached in no other way. . . . "

An "editorial" in the "Scientific American" embodied an opinion that the inventor of a reliable aërial machine would deserve to rank as one of the greatest benefactors of the human race, because, as aqueous torpedoes have almost abolished naval warfare, aërial torpedoes must lead to the abolition of war of every kind.

The third opinion I refer to is that of the Poet-laureate, in "Locksley Hall."

It seems to me that, as aërial navigation will "practically obliterate boundaries," there will be no frontiers ("scientific" or otherwise) to fight about; whilst ethnic, religious, and political jealousies must gradually disappear, "when races and languages shall be mingled as never before."

Therefore the chief causes of "aggressive war" will be annihilated by this "art of the near future."

AËRONON.

[We note the expression "ultimate results." These may perhaps be all the writer pictures. But what about the *immediate* results? We fear they would be horrors such as the world has not yet witnessed.—ED. J. S.]

NOTES.

THE occurrence of diatoms in the London Clay has been confirmed by Mr. W. H. Shrubsole, F.G.S. They were first noticed during the sinking of a well at Sheerness in 1877-8, and described in the "Proceedings of the Geologists' Association" (vol. v., p. 355); but, as the appearance of diatoms were not at that time familiar to the author, they were described as minute unknown fossils; their appearance was that of metallic specks, or, under a slight magnifying power, minute disks of iron pyrites, having a boss in the centre and the edge slightly turned up all round. In 1877 Mr. Shrubsole had an opportunity of examining a slide of *Aulacodiscus Oreganus*, and was struck with its resemblance to the unknown minute fossils. The examination of specimens of clay was renewed, samples from wells at Sheerness—of dates 1781 and 1812—examined, and diatoms discovered, but only in a narrow well-defined zone at a certain level. Further observation has shown that they exist over a wide area, but still at the same level. Most of the diatoms are pyritic pseudomorphs, but a few are obtainable which have not been completely mineralised, and will bear examination with high powers and transmitted light. The paper concludes with some observations by Mr. F. Kitton, Hon. F.R.M.S., and a list of species determined by Mr. Shrubsole, Mr. Kitton, Dr. Bossey, Dr. Stolterfoht, and Mr. G. D. Brown.

A Parliamentary Return shows the number of experiments performed on living animals during the year 1880 by medical men and others licensed under the Act. The experiments were in most cases performed at laboratories connected with the universities in England and Scotland, or at hospitals or veterinary institutions. The experiments numbered 311, of which only 114 can have been the cause of any pain, and of these all but 17 were of a kind involving no more pain than is experienced in ordinary vaccination. The painful part of the proceeding in the 17 cases involving pain was made under anæsthesia, and no appreciable suffering can be said to have been inflicted beyond confinement until the wounds healed or until the animals were killed. The most important results have been acquired in the elucidation of an obscure and fatal disease termed anthrax, which attacks sheep and cattle, and also persons engaged in wood sorting. Of this series of experiments 29 were undertaken at the instance of the Royal Agricultural Society, and 40 at that of the Medical Department of the Local Government Board.

Dr. G. W. Rachel ("Science," June 11) gives a very interesting paper on "Fossil Organisms in Meteorites." It is maintained that about fifty species of fossil Polypi, crinoids, sponges, and Algæ have been recognised in meteoric stones by Dr. Hahn, Dr. Weinland, and Prof. Karsten.

According to the "American Journal of Science" the speed of the Gulf-Stream, as it passes through the Bahama Channel, does not exceed 2½ miles per hour.

The applications of selenium to the photophone and to telephotography have been ably discussed by Mr. Shelford Bidwell in a lecture delivered before the Royal Institution. The author has explained the apparently capricious action of selenium by showing that there is a certain temperature at which it possesses a maximum resistance, diminishing rapidly as the temperature either rises or falls.

Dr. Arthur Schuster, in a lecture before the Royal Institution, has given an admirable summary of the teachings of modern spectroscopy.

The celebrated botanist and physiologist M. J. Schleiden died at Frankfort on June 22nd, in the 74th year of his age.

Prof. S. H. Scudder communicates to the "Geological Magazine" an interesting paper on two insect wings from the Dudley Coal field. To the one he gives the name of *Brodia priscotincta*, and to the other that of *Archæoptilus ingens*. The spread of wing of the latter insect must have been from 10 to 14 inches.

According to Dr. E. C. Hanson *Saccharomyces apiculatus* is an alcoholic ferment, feebler than *S. cerevisiæ*, producing bottom fermentation, but incapable of inverting cane-sugar or of throwing its solution into alcoholic fermentation. Its natural *habitat* and nutriment are gooseberries, cherries, plums, &c.

Mr. T. R. Dolan, F.R.C.S., describes in the "Medical Press and Circular" a case of hydrophobia, and remarks—"As one among many deaths that have occurred in Yorkshire during 1879, it convinces me that a more stringent Dog Act is required.

The Legislature of Massachusetts has passed an Act requiring railway *employés* to be examined for colour-blindness, and imposing a penalty of 100 dollars upon the company for each case of neglect.

We regret to announce the death of Prof. Rolleston, M.D., F.R.S., at the early age of fifty-two.

An insect plague—species not named—is said to be ravaging the agricultural districts of Lancashire.

M. E. Yung has reported to the Academy of Sciences on the influence of the nature of food upon the development of the

frog. He finds that tadpoles of one brood develope very differently, according to the nourishment which they receive. The articles used in the experiments favour the development of the tadpoles in the following order:—Beef, fish, coagulated white of hen's eggs, albumenoid matter of frogs' eggs, and Algæ. The two latter do not suffice for the transformation of the tadpole into a frog.

According to M. Jolyet the "*picote*" of pigeons is true variola.

The liquid ejected by the oak eggar (*Bombyx Quercus*), on emerging from the pupa, is opaque, of a yellowish buff colour, and has a well-marked acid reaction.

H. Horvath ("Physic. Med. Gesellsch. Würzburg") concludes an experimental investigation on the hybernation of animals with the decision that "hybernation is not sleep, and has no connection with winter."

Prof. Seeley ("Geological Magazine") criticises Prof. Carl Vogt's views on the *Archæopteryx*. He rejects the supposition that this species forms a marked intermediate type between birds and reptiles.

According to reports collected by Mr. Glashier, extending over thirty-two consecutive years, the proportion of dry warm seasons during the hay and corn harvest is about one in ten. This fact shows the importance, or rather the necessity, of the Gibbs hay and corn drying machine.

We are happy to learn that the Perthshire Natural-History Museum has become a reality. The institution is to be confined to the zoology, botany, and geology of Perthshire. We could wish that a similar museum for local Natural History could be founded in every county.

Prof. Semper has been examining the effect of differences in light upon young axolotls. In the dark they do not become white, but very dark; almost as dark in red light, rather paler in yellow light, and palest of all in full daylight.

The "Scottish Naturalist" for July contains the continuation of a most interesting biography of the meritorious Scottish botanist George Don.

The same journal gives a list of many plants, in the neighbourhood of Leith, which have either been killed or greatly damaged by the past severe winter.

Among the many and discordant voices now raised against Science must be counted that of Mr. Ruskin, who ("Fors Clavigera," Letter 53, pp. 138, 139) is guilty of this strange utterance:—"The instinct for the study of . . . the lower forms of

undeveloped creatures . . . is the precise counterpart of the forms of idolatry (expressed in the worship of unclean beasts) which were in great part the cause of final corruption in ancient mythology and morals."

We wish to call special attention to an Exhibition of appliances, fuels, &c., for the prevention of smoke in dwelling-houses and factories, to be opened on October 24th, under the presidency of H.R.H. Prince Leopold, Duke of Albany.

Mr. H. H. Howorth ("Geological Magazine") argues that the circumstances under which the remains of the mammoth are found in Siberia, prove that these animals must have been destroyed by a great, sudden, and permanent fall of temperature.

Prof. Hutchinson, in his Lectures on the "Laws of Inheritance in Relation to Disease," mentions the fact that colour-blindness is exceptionally common among Jews and Quakers.

The "Medical Press and Circular" justly remarks that "Of the rank and file of the æsthetic army, as of every hysterical school before, the majority are men of feeble intellectual development, and women of that ultra-sympathetic type which is one of the forms of disease most prone to excite pity and disgust in the mind of the physician."

Prevention of Forgery.—In our February issue we gave an account of the safety-paper devised by Mr. A. Nesbitt, F.C.S., to prevent fraudulent alterations in cheques. We now learn that another process for the same object has been brought forward by Dr. Dupré and M. Hehner, who appear to be chemists to the firm of Charles Skipper and East, cheque-printers. These gentlemen propose to print upon the cheque a mixture of zinc sulphide and lead carbonate; and they state that on treating the cheque with an acid or an alkali calculated to remove writing, double decomposition takes place, and the paper is darkened by the lead sulphide produced. This result undoubtedly happens with sulphuric acid at certain strengths. But if the *right* degree be hit, as we have seen it experimentally proved, lead *sulphate* is produced instead of *sulphide*, and the mixture and the paper to which it is applied remain white as before. Again, if alkaline solutions are used at random the mixture turns a brownish colour, and the attempt at fraud is betrayed. But if an alkaline solution is used of the proper strength, and containing a suitable proportion of sodium bicarbonate, the mixture of zinc sulphide and lead carbonate remains perfectly white, and the writing gradually disappears. Hence this invention would afford no protection against a clever forger, who would experiment till he had found the exact strength of the solutions to discharge the writing without blackening the paper.